Popular Astronomy

Vol. LIII, No. 2 **FEBRUARY, 1945** **Whole No. 522**

The Cause of Sun-Spots

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Introduction. Schwabe's solar observations from 1826 to 1859 established the sun-spot cycle. The researches of Wolf set its mean length at 11.1 years. This was increased to 11.2 years by the maximum of 1917.6. This last is not necessarily the final definitive value since from the maximum of 1836.5 to that of 1937.2 the lengths of the cycles are:

11.9 years	12.8 years	11.2 years
12.0 years	10.4 years	10.9 years
10.4 years	12.4 years	10.1 years

Here five cycles are less than Jupiter's period of 11.86 years and four are greater than it. In the light of these facts it would be very unscientific to assume that Jupiter has nothing to do with the sun-spots.

It is the aim of this paper to offer new and, what is believed to be, conclusive evidence that the sun-spots are caused mainly by the precessional action on the equatorial belt of the sun by the planets, Mercury, Venus, Earth, Jupiter, and Saturn.

In August, 1930, the writer published in the *Astronomical Journal* an article entitled "On the Cause of the Sun's Spots." The cause proposed was the precessional action mainly of Jupiter and Saturn on the sun. No attempt was made to show that Mercury, Venus, and the Earth were also effective in producing spots. In December, 1940, POPULAR ASTRONOMY published an article of mine entitled "Planets and Sun Spots." Section 6 of that paper indicated briefly a method of proving that the precessional action of the Earth registered in the sun-spot record. That method is here detailed and extended to the five planets named in the preceding paragraph.

2. *Precessional Action.* The essential features of precessional action are easy to understand. They must register, however, if what follows is to be clear. Precessional action on any body is the result of two equal and opposite forces called a couple. It produces, or tends to produce, rotation. A couple has no single resultant and only an equal opposite couple can stop the tendency to rotation. The magnitude of a couple is measured by the product of one of the two forces and the length of the arm at which they act.

Tisserand ("Mecanique Celeste," Vol. II, pp. 92-94) and Moulton (*Astrophysical Journal,* Vol. 29, p. 260) both have proved that the equatorial diameter of the sun is greater than the polar diameter. All attempts to establish this observationally have failed (Abetti, "The Sun," pp. 82-84). According to Moulton the difference between the two diameters is 31.54 miles. That would make the *volume* of the solar bulge about 100 times the volume of the earth.

The density of the bulge is not known but the surface gravity of the sun is 27.65 times that of the earth. Hence we will not be far wrong if we assume the density of the solar bulge to be .000001 of the sun's mean density. Calculation, using Formula (1) of Section 3 then gives for the couple of Jupiter on the sun 6.653×10^{11} kilometer metric tons. From Table I, Section 3, the combined couple of all planets is about three times that of Jupiter. That would be 2,000 million metric tons acting at the end of an arm 1000 kilometers long. This gives some idea of the magnitude of the forces at work.

Beginners in astronomy learn that the earth's equatorial diameter is over 26 miles greater than the polar. The moon's attraction on the near and far portions of the belt form a couple which tends to shift the plane of the earth's equator into the plane of the moon's orbit. But the earth is, so to speak, a rigid body and the equatorial bulge is firmly attached to it. Hence the net result is a continual shift in the direction of the plane of the equator accompanied by a conical motion of the earth's axis. Thus the earth spins *very slowly* like a top. Obviously the sun also exerts a couple and contributes to the resultant spin. It may be added that there would be no spin if the earth were a perfect sphere.

What Jupiter does may be described in simple terms as follows. Jupiter acts on the sun's equatorial bulge, which is gaseous and not rigidly attached to the body of the sun, and tends to shift the bulge into the plane of the planet's orbit. The sun's rotation forms the bulge and tends to maintain it in the same position with respect to the sun's axis. The interaction of these two sets of forces disturbs the solar equilibrium continuously. The result is not a minute spin of the sun like a top but a profound disturbance of the material in the thicker portion of the equatorial bulge. This is the cause of the sun's spots.

3. *Fundamental data.* The astronomical data of Table I are the basis of this whole paper. Formula (1) plays a vital role in the reasoning.

TABLE I

(1) Planet	(2) Inclination of orbit to sun's equator	(3) Longitude of ascending node of sun's equator on plane of orbit	(4) Longitude of perihelion 1930	(5) $\frac{(1+e)^3}{(1-e)^3}$	(6) Mean Couple Earth = 1	(7) Extreme values of couple	
Mercury	3°14′	327°34′	76°21′	3.480	.3250	.59,	.17
Venus	3 47	251 50	130 35	1.041	1.138		
Earth	7 11	253 47	101 44	1.138	1.00	1.051,	.951
Mars	5 34	261 50	334 46	1.753	.024		
Jupiter	6 2	248 20	13 11	1.301	1.89	2.18,	1.71
Saturn	5 29	237 5	91 40	1.398	.083	.0978,	.0668
Uranus	6 25	253 48	169 31	1.329	.00186		
Neptune	6 23	240 14	44 1...	1.056	.00054		

Webster's "Dynamics," page 301, gives the following formula for the magnitude of the precessional couple:

$$3\ km \times (C - A) \sin l \sin d \sqrt{(1 - \sin^2 l \sin^2 d)}/r^3 \qquad (1)$$

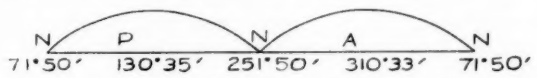
Here

k = the constant of gravitation.
m = the mass of the planet.
C — A = difference of the principal moments of inertia of the sun.
l = the longitude of the planet measured from the node of the sun's equator on the orbital plane.
d = the angle the sun's equator makes with the orbital plane.
r = the distance of the planet from the sun.

4. *Analyses of the Sun-Spot Data.* Numerous analyses of the sun-spot data have been made by various investigators. The purpose was to discover from them what component periods existed. The hope was that the periods found would lead to the discovery of the cause of the spots themselves. For reasons which will not be detailed none of the analyses were successful in this aim.

Preceding analyses of sun-spot data were carried out in absence of any known cause of the spots and in ignorance of all secondary periods. The latter were the result of the process used. In the present analysis dynamic reasoning indicated the secondary periods which should be present. It was necessary to devise a method which would show their presence. The procedure while laborious is simple and easily understood as it involves arithmetic only.

5. *Venus and the Sun-Spot Record.* Column 6 of Table I shows that the magnitude of the precessional action of Venus comes next to that of Jupiter, being equal to 60 per cent of it. Consequently, its action should be easily detected in the sun-spot record. To understand how this may be done the adjacent graph is helpful.



VENUS—PRECESSIONAL COUPLE

In one 225-day revolution of the planet its precessional action has two minima and two maxima. The minima occur at the nodes as the planet is then in the plane of the sun's equator and sin l, of formula (1), is zero. The maxima would come midway between the nodes were it not for the inversely varying cube of the distance, r, of the planet from the sun. This displaces the maximum slightly toward the perihelion point, P, and slightly away from the aphelion point, A.

To detect the period of Venus in the sun-spot record the 225-day period was divided into 15 parts of 15 days each. Then the sums of the sun-spot numbers for each successive 15-day interval were obtained from the *daily* sun-spot record. The range was from January 1, 1916, to April 29, 1936, comprising 33 revolutions of Venus. In all, 495 items were calculated from the sun-spot record. These items were

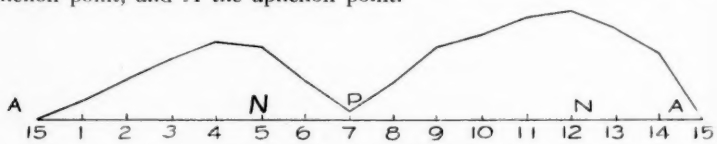
then ordered in fifteen columns with 33 numbers in each column. The fifteen sums are given in column A, Table II.

For convenience in making the 15-point graph, the least number 19434 was subtracted from each of the fifteen. The results are in column B. This practically eliminates the effect of all the planets but Venus. Each number in B is the cumulative effect of 33 equal impulses of Venus over its 225 day period. The calculations were so made that the first point of the graph comes at January 1. The curve is the graph of the numbers in B. The critical positions of Venus can be taken from the *Ephemeris* for any one of its revolutions. Those given are for the first revolution of Venus in 1916.

TABLE II

	A	B			A	B		
Jan. 1	19839	405	(1)		Apr. 30	21400	1966 (9)	
Jan. 16	20412	978	(2)		May 15	21722	2288 (10)	
Jan. 31	20959	1524	(3)		May 30	22241	2807 (11)	
Feb. 15	21486	2052	(4)	Node	June 14	22307	2873 (12)	Node
Mar. 1	21273	1839	(5)	Feb. 24	June 29	21843	2409 (13)	
Mar. 16	20414	980	(6)		July 14	21150	1716 (14)	Aph.
Mar. 31	19489	55	(7)	Per.	July 29	19434	0 (15)	July 22
Apr. 15	20481	947	(8)					

In all the graphs which follow, N denotes nodal points, P the perihelion point, and A the aphelion point.



VENUS—SUN-SPOT PRODUCTION
1916-1936, 33 REVOLUTIONS

The 15-point graph has two minima and two maxima. *The minima follow the nodes.* This shows a lag of the effect behind the cause. The curve shows unmistakable agreement with the precessional action of Venus. Inertia of the disturbed material would be expected to carry the effect beyond the point at which precessional action is zero.

To guard against errors and chance agreements a similar study was made over the interval January 1, 1870, to January 9, 1894, a total of 39 revolutions of Venus. The *daily* sun-spot numbers published in the *Journal of Terrestrial Magnetism and Atmospheric Electricity* begin in 1916. For this second interval the monthly means were used. These are the average number of spots per day for each month of the year. The orbital period was separated into 9 parts of 25 days each. The procedure used for the first three months of 1870 may be indicated as follows:

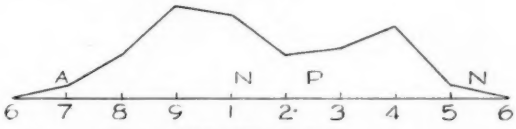
MONTLY MEAN

Jan.	77.3	$25 \times 77.3 = 1932.5 =$ Part 1
Feb.	114.9	$6 \times 77.3 + 19 \times 114.9 = 2646.9 =$ Part 2
Mar.	159.4	$9 \times 114.6 + 16 \times 159.4 = 3581.5 =$ Part 3

etc., for six more parts completing one revolution. This was carried out for 38 more revolutions. In this way a total of 9×39 or 351 items were taken from the sun-spot record. These were ordered in 9 groups of 39 numbers each. The sums of each 39 are in column A, Table III. The same procedure as before gives the 9-point graph.

TABLE III

	A	B	
Jan. 1	44952	2263 (1)	
Jan. 26	43702	1013 (2)	Node Jan. 3
Feb. 20	43950	1261 (3)	
Mar. 17	44628	1938 (4)	Per. Feb. 8
Apr. 11	42965	276 (5)	
May 6	42689	0 (6)	Node Apr. 24
May 31	42952	263 (7)	Aph.
June 25	43535	846 (8)	
July 20	45176	2487 (9)	



VENUS, 1870-1894, 39 REVOLUTIONS

This graph also has two maxima and two minima. The latter occur soon after Venus passes the preceding node. This graph shows the same significant agreement with the precessional action of Venus as the first one. The positions of aphelion and perihelion show why the minimum near aphelion is more marked than that near perihelion.

It is difficult to reduce the sun-spot numbers from the many observers so that they are homogenous. Moreover, there are marked disagreements between them and the area numbers. These are inevitable deficiencies in the methods of measuring the solar action. Hence exact parallelism can not be expected between the precessional curve of Venus and the two preceding sun-spot curves. The *two minima* coming just after the zeros of precessional activity are significant and conclusive agreements. So also are the *two maxima* which are always present.

6. *Mercury and the Sun-Spot Record.* The adjacent figure is a

MERCURY—PRECESSIONAL COUPLE

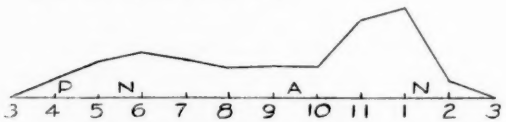
graph of the precessional action of Mercury showing the position of the four critical points: the two nodes, perihelion, and aphelion. It is very different from that of Venus. The eccentricity of the orbit is high 0.2056. Consequently, from node to node through perihelion is 33 days while the interval from node to node through aphelion is 55 days. The maximum couple in each arch tends to occur midway between the

nodes but is displaced slightly toward perihelion and away from aphelion.

The procedure used with Venus was followed with Mercury. The interval selected for study included the 50 revolutions of Mercury following January 1, 1916. The orbital period of 88 days was divided into 11 parts of 8 days each. From the daily sun-spot record the sums for each eight days of the entire interval were calculated. This gave a total of 550 items. The results were ordered in eleven columns and the sum of each 50 items was found. The same procedure as for Venus gave the following data (Table IV) and the eleven-point graph. Again the calculations were so made that the first point of the curve is January 1.

TABLE IV

		A	B		
Jan.	1	20417	2350	(1)	Node Jan. 5
Jan.	9	19363	296	(2)	
Jan.	17	18067	0	(3)	Per. Jan. 26
Jan.	25	18495	428	(4)	
Feb.	2	19026	959	(5)	Node Feb. 7
Feb.	10	19251	1184	(6)	
Feb.	18	19027	960	(7)	
Feb.	26	19201	834	(8)	
Mar.	5	18827	760	(9)	Aph. Mar. 10
Mar.	13	18896	829	(10)	
Mar.	21	19979	1912	(11)	



MERCURY, 1916-1929, 50 REVOLUTIONS

The curve shows two maxima and two minima. The effect lags behind the cause. Hence the two minima follow respectively the two nodes which are the two zeros of precessional action.

The curve is so different from the curves for Venus that there is a sharp feeling of surprise. According to Formula (1), Section 3, precessional action at perihelion is about 3.5 times as great as at aphelion. Why is this not reflected in the curve? It appears that the delayed minimum at point 3 of the curve is partly responsible. It seems also that the 33-day period is too short to permit Mercury's greater precessional period to register fully in the spot curve. In contrast the 55-day period allows the smaller couple to show a marked effect.

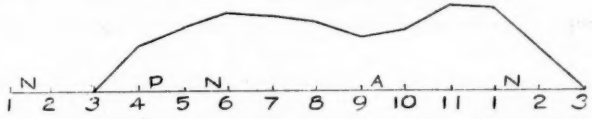
A further study of Mercury was made for an additional fourteen years previous to January 1, 1916; that is, back to January 1, 1902. The daily sun-spot numbers were not available so the eight-day sums were calculated from the monthly means. The whole interval from 1902 to 1928 covers 109 revolutions of Mercury; that necessitated finding 11×109 or a total of 1199 eight-day sums from the sun-spot record. Since the critical positions of Mercury can be taken from any

one of its revolutions the first one in 1916 was used. The sums for each interval and the numbers for the whole range 1902-1928 follow, Table V. The graph is for the 109 revolutions.

TABLE V

MERCURY 1902-1928 (109 REV.)

		1902-1916	1916-1928	1902-1928		
Jan.	1	13714	20417	34131	2264 (1)	Node Jan. 5
Jan.	9	13660	19363	33023	1156 (2)	
Jan.	17	13800	18067	31867	0 (3)	
Jan.	25	14201	18495	32696	829 (4)	Per. Jan. 26
Feb.	2	14430	19026	33456	1589 (5)	Node Feb. 7
Feb.	10	14659	19251	33910	2043 (6)	
Feb.	18	14782	19027	33809	1942 (7)	
Feb.	26	14598	19021	33619	1752 (8)	
Mar.	5	14499	18827	33326	1459 (9)	Aph. Mar. 10
Mar.	13	14586	18896	33482	1615 (10)	
Mar.	21	14226	19979	34205	2338 (11)	



MERCURY, 1902-1928, 109 REVOLUTIONS

It would seem that such marked results for Venus and Mercury would not have entirely escaped observation. As a matter of fact they did not, but the observations have been disregarded by the astronomical world or else looked on as doubtful.

Mrs. Maunder (*M.N.R.A.S.*, V. 67, pp. 451-479, 1906-1907) in her study of sun-spots found that as the sun's rotation brings new parts of the solar surface into view those parts bear more than the average number of spots. She concluded that the earth caused the number of spots to decrease. The correct reason, as the graphs for Venus and Mercury prove, is that the effect lags behind the cause.

Pocock studied spots in the interval 1902-1917 and confirmed Mrs. Maunder's results (*M.N.R.A.S.*, V. 79, November, 1918). De La Rue, Stewart, and Loewy found for Venus and Mercury the same result as Mrs. Maunder and Pocock did for the earth.

Schuster using only new spots reported on daily photographs, investigated the interval 1874 to 1909, and found that Mercury, Venus, and Jupiter all appear to have an influence on spot production. (*Proceedings of the Royal Society of London*, Vol. 85, pp. 309-323). Schuster concludes that the probability of the observed coincidences being accidental is one in 400,000.

Here is a considerable array of correct and significant observations. One reason that they have gone comparatively unnoticed is because the observers did not offer any explanation of the dynamical method by which the planets caused the spots.

There is a dynamical basis for precessional action as a cause of sun-

spots which may be outlined as follows:

First: There is the magnitude of the precessional couple of Jupiter on the sun which, by Formula 1 of Section 3, is of the order 1.47×10^{22} gram centimeters. This is small compared to 2.81×10^{29} gram centimeters, which is, according to Lord Kelvin, the moon's couple on the earth.

Second: Poincare has proved (Lamb, "Hydrodynamics," pages 677-680) that a fluid body will precess like a rigid one provided the period, P, of the disturbing body divided by the period, p, of the disturbed body is large compared to

$$(a^2 + c^2)/(a^2 - c^2)$$

of the disturbed body, that is:

$$P/p > (a^2 + c^2)/(a^2 - c^2) \text{ or } > [(r + x)^2 + r^2]/[(r + x)^2 - r^2] \text{ or } > r/x$$

approximately. (1)

Here r is the sun's polar radius, $r + x$ is its equatorial radius, and x according to Moulton is 15.77 miles. For the sun and Jupiter (1) becomes:

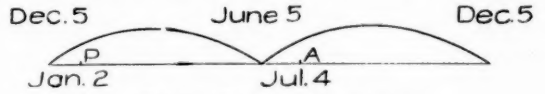
$$(11.86 \text{ years})/(24.65 \text{ days}) > (433200/15.77) \text{ or } 175.7 > 27470 \text{ or } 1 > 156 \quad (2)$$

Instead of the left member of (2) being greater than the right, it is far smaller and Poincare's condition is *not* fulfilled; that is, there is no possibility of the slightest axial spin of the sun. Hence the precessional action of Jupiter on the sun, which is considerable, must be expended in causing action of some type in and below the equatorial bulge. This is the dynamical foundation for sun-spots and also for the belts of Jupiter.

Third: There is also observational evidence that precessional action of the moon on the waters of the Atlantic Ocean has been observed in the motion of the Gulf Stream. This was established through a four year study by Lieutenant (later Rear Admiral) J. E. Pillsbury. He spent about a year on each of four sections of the Stream, anchored his boat and measured the flow at many points in each section. His calculations showed that 21 cubic miles of water flow through the Straights of Florida every hour. Moreover, and this is a significant discovery bearing on solar study, he found that twice in every lunar month, at high declination of the moon and at low declination, the character of the flow changes. (See Report of U.S. Coast and Geodetic Survey for 1893.)

High tide occurs at new and full moon and these do not occur regularly at high declination of the moon and at low declination. The greatest precessional action of the moon does occur at these times. Hence the result Pillsbury observed is due to precessional action of the moon on the waters of the Atlantic. The causes of the flow of the Gulf Stream are well known and no one lists precessional action of the moon (or the sun) as one of them. The slight variation of the flow noted is merely an index of precessional action.

7. The Earth and the Sun-Spot Record. Table I, column 6, shows that the precessional couple for the earth is less than that of Venus. The variations in precessional action are not so marked as for Venus for two reasons. First, because the magnitude of its couple is less than that of Venus and, second, as the figure below shows, perihelion and



EARTH—PRECESSIONAL COUPLE

aphelion are both near a node. Were each midway between the nodes the difference in the two maxima of precessional action would be large. As it is the difference is small and one is displaced slightly toward perihelion and the other slightly away from aphelion.

Calculation shows that the most active month is February and the next most active is September, but the difference in activity for August and September is small. The minimum of June 5 should be marked because the node and the aphelion are very near together. The node of December 5 is only 28 days from perihelion. Consequently, precessional action during November is nearly equal to that during December.

8. *The Area Numbers.* The sun-spot numbers and the Greenwich area measures are in good agreement in fixing the epochs of maximum and minimum solar activity. In an investigation such as this the area numbers are very inconvenient to use because the means are calculated for synodic revolutions of the sun instead of for calendar months. The length of a synodic revolution of the sun varies slightly throughout the years. For the interval 1874 to 1913, inclusive, however, Arctowski (*Memoria della Societa Spettroscopisti Italiani,* Vol. V, pp. 98-99) made the necessary reductions of the area numbers to means for calendar months. The sums of the 40 means for each of the twelve months of the year are given in Table VI.

TABLE VI

AREA NUMBERS 1874-1913

Jan. 501	May 452	Sept. 553
Feb. 535	June 413	Oct. 519
Mar. 484	July 526	Nov. 481
Apr. 451	Aug. 495	Dec. 497

The agreement with precessional action is good. The minima come in June and November and the maxima in February and September.

The sun-spot numbers over the same 40-year interval give about the same results as the area numbers, as shown in Table VII.

TABLE VII

Jan. 1261	May 1257	Sept. 1352
Feb. 1389	June 1303	Oct. 1311
Mar. 1295	July 1417	Nov. 1132
Apr. 1320	Aug. 1342	Dec. 1191

The sums of the monthly sun-spot means for the 100-year interval 1839 to 1938 are:

TABLE VIII

Jan. 4310	May 4490	Sept. 4580
Feb. 4690	June 4540	Oct. 4500
Mar. 4500	July 4540	Nov. 4400
Apr. 4350	Aug. 4670	Dec. 4410

Here the agreement with precessional action would be good were it not for the four summer months in which the numbers are too great. This is probably a seasonal effect due to the better "seeing" of the summer days and the greater number of hours of observation per day.

Mars, because of its small couple, will not be considered. The results for Mercury, Venus, Earth, Jupiter, and Saturn will be sufficient to complete the purpose of this investigation.

9. *Jupiter and the Sun-Spot Record.* Jupiter's orbital period of 11.862 years was divided into twelve equal parts of 0.988 year each. The sun-spot numbers for each 0.988 year were calculated and ordered into groups of twelve. Then the corresponding numbers for each of the twelve parts were added. The results follow.

Table IX shows Jupiter's sun-spot activity in each of 12 parts per orbital period. Two different intervals from 1834 to 1941 are given. The first is 1858 to 1942; the second is 1834 to 1942, inclusive.

TABLE IX

Orbital Parts	1	2	3	4
84 yr. 7 rev.	120.9	154.9	283.6	400.3
108 yr. 9 rev.	267.6	288.8	375.11	461.27
Orbital Parts	5	6	7	8
84 yr. 7 rev.	505.4	574.7	444.4	320.6
108 yr. 9 rev.	577.9	664.0	621.0	581.1
Orbital Parts	9	10	11	12
84 yr. 7 rev.	226.1	139.9	128.2	192.4
108 yr. 9 rev.	446.2	303.3	271.0	302.8

Subtracting 120.9 from each of the twelve values in line one gives the residuals which form a measure of Jupiter's sun-spot activity:

0 34 162.7 279.4 384.5 454 324 200 105 19 7.3 71.5

Reducing these for plotting:

0 .68 3.3 5.6 7.7 9.1 6.5 4.0 2.1 1.6 .14 1.43

These last give the first of the adjacent graphs. The numbers in line two, Table IX, give the residuals:

0 21.2 107.5 193.7 309.7 396.4 353.4 313.5 178.6 35.7 3.4 35.4

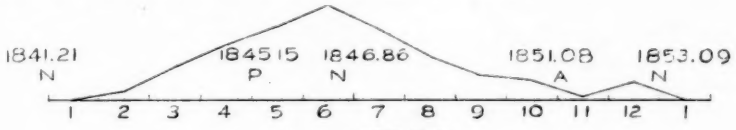
Reducing these for plotting:

0 .53 2.74 4.84 7.74 9.16 8.84 7.84 4.46 .9 .09 .88

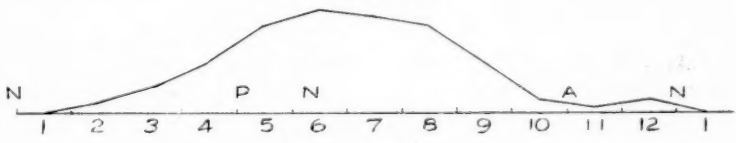
These numbers give the second graph.

In the first graph of Jupiter's action two maxima and two minima do *not* appear. For this the positions of the nodes, the perihelion and aphelion points are partly responsible. In addition the enormous couple of Jupiter and the lag of the effect after the cause contribute. The first

precessional impulse is 5.63 years long and is far greater than the second. From the node the action increases until about one and one-half years after perihelion passage. The magnitude of the forces present and the enormous area over which the activity is spread have a certain inertia which produces a considerable lag. The effect of both nodes is to turn the curve downward.



JUPITER, 1858-1942



JUPITER, 1834-1942

The second precessional impulse is 6.23 years long and far less than the first. It supports the activity, however, until it diminishes almost to zero at aphelion. After aphelion there is a slight rise; then the passage of the node brings zero again. The behavior of the curve *here indicates small but significant and conclusive agreements.*

The second graph of Jupiter's action may be interpreted in like manner. In this way the two precessional impulses of Jupiter are merged into a single one. This brings us to Newcomb's considered conclusion:

"Underlying the periodic variations of spot activity, there is a uniform cycle unchanging from time to time and determining the general mean of the activity" (*Astrophysical Journal*, Vol. 13, 1901, pp. 1-14).

10. *Saturn and the Sun-Spot Record.* Four times Saturn's period of 29.46 years is 117.84. The interval of reliable sun-spot observations from 1826 to 1943, inclusive, is 118 years. In that time, Saturn has made *four* complete revolutions around the sun. This is not great enough for the best results; nevertheless, definite confirmation of precessional action by Saturn can be obtained from these four revolutions.

The Wolf sun-spot activity of Saturn as measured by the sums of corresponding four-year means for each of the 29.5 years of the period is indicated by numbers as follows:

TABLE X

30.6	40.8	54.6	25.8	57.2
35.3	38.3	60.3	30.1	50.3
48.7	38.4	63.3	40.4	41.6
60.1	46.3	55.5	55.5	33.3
61.1	59.9	40.5	72.8	25.7
48.0	61.5	29.0	65.3	18.6 (29.5)

These results are rather irregular but if we smooth them by taking overlapping nine-year means their definite trend appears.

The nine-year means are in the first three columns which follow. Subtracting 42.2 from each of these gives the residuals in the last three columns, Table XI. These form a measure of Saturn's spot-producing action.

TABLE XI

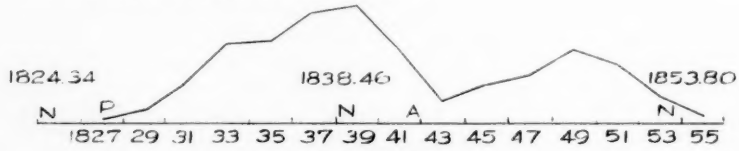
(1826)	42.2	51.6	46.3	0.0	9.4	4.1
	42.5	53.3	47.3	0.3	11.1	5.1
	42.8	53.4	48.8	0.6	11.2	6.6
	43.8	52.3	49.6	1.6	10.1	7.4
	44.6	50.0	49.2	2.4	7.8	7.0
	46.4	46.7	48.4	4.2	4.5	6.2
	49.1	44.4	44.9	6.9	2.2	2.7
	50.5	44.5	44.4	8.3	2.3	2.2
	49.9	45.9	44.1	7.7	3.7	1.9
	49.8	46.1	42.2	7.6	3.9	0.

Taking the means of each pair of adjacent numbers in the last three columns gives:

TABLE XII

1827	.15	1837	10.2	1847	4.6
1829	1.1	1939	10.7	1949	7.0
1831	3.3	1841	6.2	1851	6.6
1833	7.6	1843	2.3	1853	2.5
1835	7.65	1845	3.8	1854.5	.95

The fifteen-point graph of these follows.



SATURN, 1826-1943

The sun-spot curve of Saturn agrees with its precessional curve in having two minima and two unequal maxima. The effect of the nodes and aphelion is clearly reflected in the curve. The two precessional periods of Saturn are, respectively, 14.12 years and 15.34 years long. Calculation shows that the ratio of the activity of the shorter period to the longer is about 11 to 8. The magnitude of the forces involved and the enormous area over which spot activity is spread produce a considerable lag in the maxima and the minima. The period of Saturn is about 50 times that of Venus. Hence a much greater lag for Saturn than for Venus would be expected.

Periodogram Analyses. The existence of the periods found in this study have all been confirmed by the periodogram analyses of three independent investigators. In 1891 Schuster published in a Vienna journal the result of an examination of the interval 1880 to 1887 for *short periods,* and found one of 69.4 days. This corresponds to the interval from conjunction to opposition of Mercury and Venus which is 72 days.

Elsa Frankel sought *short periods* in the sun-spot data over the intervals 1880-1887, 1890-1899, and 1902-1911. She found a period of 68.5 days and one of 200 days. (*Pub. der Sternwarte des Eidg. Polytechnikunst zu Zurich,* Bd. V, S. 47.) Her first result confirms that of Schuster, to whose work she refers in the introduction to her paper. The second, 200 days, corresponds to the mean interval from conjunction of the earth and Jupiter to their opposition, which is 199.5 days.

The precessional action of Mercury, Venus, Earth, and Jupiter is confirmed by these investigations. Neither Schuster nor Elsa Frankel had any idea of what the periods they found meant. Elsa Frankel said they reminded her of the 88-day sidereal period of Mercury and the 225-day sidereal period of Venus.

Other short periods exist which have not been discovered by any periodogram analysis are:

Mercury-Earth	58 days	Venus-Earth	292 days
Venus-Jupiter	119 days	Mars-Jupiter	1.19 years

The Venus-Jupiter period is the strongest of all the short periods. Elsa Frankel said her work indicated another short period which she would have to defer to second research. These facts are of great significance for Dr. Nicholson points out:

"Averaged by months, they show that the cycle does not progress in a regular fashion but by a series of fluctuations which vary in lengths from five or six to as much as fifteen months. These fluctuations are smoothed out when the yearly means are taken." (The Solar Cycle, Leaflet No. 50, *P.A.S.P.*)

For long-period effects Clayton has the most striking results by what he calls harmonic analysis. Over the interval 1793 to 1936 he obtained among others the following periods in years: 9.93, 19.86, 11.17, 11.90, and 14.89 (*Smithsonian Miscellaneous Collections,* Vol. 98, Number 2). The synodic period of Jupiter and Saturn is 19.86 years and the interval from conjunction to opposition is half of this or 9.93 years, which is exact agreement. It may be noted in support of the 9.93 year period that Alter and Stumpf each obtained a 10-year period by periodogram analysis. The writer also obtained a period of 9.75 years from the area measures of the interval 1832 to 1935. The 11.17-year result is the standard mean length of the sun-spot cycle. Corresponding to 11.90 is Jupiter's sidereal period of 11.86 years. Half Saturn's sidereal period is 14.73 years which corresponds to the 14.89-year period found.

Thus three independent periodogram analyses confirm the precessional sun-spot activity of Mercury, Venus, Earth, Jupiter, and Saturn.

11. *Conclusion.* A simple procedure for five planets shows that each is effective in producing sun-spots. The curves obtained prove for each planet that its orbital period and its precessional period register in the sun-spot data. Each of the five curves shows what the underlying

dynamics is. The cause of the sun-spots is the precessional action of all the planets on a slightly ellipsoidal sun. Moreover, the dominant actor is Jupiter. Thus two road blocks to solar knowledge, a 7-year spot-cycle and a 17-year one, are now removed.

It will be illuminating to consider how the dynamics of precession works on the sun. An easily understood phenomenon of sun-spots is the solar equatorial belt which is nearly free of spots. The plane of the sun's equator makes an angle of 6° 2′ with the plane of Jupiter's orbit. For half a solar rotation with this 12 degree belt whatever motion is imparted to the material at and near the solar surface by Jupiter's precessional action is reversed during the next half rotation. Outside this belt the action is cumulative. Here is the reason why spots are "infrequent" from solar latitude 5° north to 5° south.

Three other crucial solar phenomena, the presence of *two* sun-spot belts, Spoerer's law, and the equatorial acceleration of the sun have been explained on the basis of precessional action in sections 11 and 12 of my 1930 article in the *Astronomical Journal*. Those explanations will not be repeated here. Section 10 of that paper also gives a much needed account of the sun-spot data before 1826.

If precessional action of the planets can produce sun-spots on a slightly ellipsoidal sun, conversely precessional action of the sun on certain planets should be discernible. Jupiter's equatorial diameter is nearly 6,000 miles greater than the polar. Jeffries places its gaseous envelope at about 4,000 miles deep. Jupiter's period of rotation is 9 hours and 50 minutes. It is not surprising, therefore, that the sun's precessional action on this planet produces *not spots* but ever changing continuous *belts*. Moreover, Jupiter's surface has a marked equatorial acceleration.

Confirmative evidence comes from long continued observations of Jupiter which have discovered two color changes in the belts. For the "tawny color," the period is 11.86 years, the planet's orbital period and it runs from equinox to the same equinox again. For the oppositely changing redness of the two belts, it reaches equality at the solstices and the two periods would be the precessional periods. (See article by the writer "On the Cause of Jupiter's Belts," POPULAR ASTRONOMY, November, 1929).

Similarly, Saturn has belts and its surface shows equatorial acceleration. Belts have been observed on Uranus and Neptune also but these planets are too far away to learn whether equatorial acceleration of their surfaces is present or not. Here are four additional instances of precessional action in the solar system which involve motions other than a conical spin of the axis of the rotating body.

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Personal Equation in Astronomy*

By RAYNOR L. DUNCOMBE

(Continued from page 13)

SECTION III

Experimental Psychology and Personal Equation

Experimental psychology, a relatively new science, developed in the latter half of the nineteenth century mainly from a confluence of the streams of modern philosophy and of experimental physiology. The contribution of philosophy stems principally from the British school of philosophical psychology as represented by John Locke, James Mill, and John Stuart Mill. This philosophy was based on empiricism; that is, it regarded individual experience as the basis for the genesis of the mind. Diametrically opposite was the German school of Leibnitz, Kant, and Fichte, which propounded intuitionism. Physiology gave the new science a background of study of the brain, the nervous system, of sensation, and most important, the experimental method.

The three men who stand out as the founders of experimental psychology in the nineteenth century are Fechner, Helmholtz, and Wundt. Prior to their work, psychology was regarded as being in the realm of philosophy and also of physiology; with them it became a science in its own right. Briefly, Fechner perfected the Weber-Fechner law on the relation of stimulus and sensation, Helmholtz experimented with sense-physiology and measured the rate of nervous impulse, while Wundt proposed the first complete system of psychology and established at Leipzig a laboratory to verify experimentally the tenets of his system. To this new science, the oldest also made a contribution, for astronomy gladly gave to the new experimental psychology the problem of personal equation, with extensive data, a method, and a few ideas.

The history of psychology since the time of Wundt deals with a series of different schools of thought, each with its own system in vogue for a period of time. Titchener, a pupil of Wundt, founded the American structuralist school, against which the functionalist group, led by Dewey and Angell, arose in protest at the beginning of the present century. About 1910 the German Gestalt school and American behaviorism appeared on the scene to supplant earlier systems. The "purposive" tenets of McDougall and Freudian theory also appeared, but in the last two decades open-minded psychologists have tended to accept the good points of all these schools, rather than succumb completely to any one dogma.

When the new experimental psychology undertook to analyze, about

*Note: The references in this paper will be given in chronological order at the end of the final installment.

1860, the bothersome astronomical question of personal equation, no distinction was made between the Bradley method and the key-and-chronograph method of observing; both were classed merely as reaction experiments. As a result, much of the early work at Wundt's laboratory in Leipzig centered around the measurement of simple reaction times.

The visual-auditory complication reaction, as Wundt later came to call the Bradley method, could not be explained by the mere measurement of simple reaction times, however, and it was not until 1885 that von Tschisch, a pupil of Wundt's, discovered the importance of attention in this experiment. Wundt described attention as being composed of a *Blinkfeld* and a *Blinkpunkt,* an attention field and an attention point or focus. Von Tschisch found in the Bradley method that the predisposition of the focus of attention could change the apparent order of stimuli by changing the order of perception. It is remarkable that Bessel, in 1822, very nearly stated the importance of attentional predisposition when he noted that one observer might go "over from seeing to hearing, and the other from hearing to seeing."

The method Wundt used for studying the complication reaction seemed practical and direct. It consisted of breaking down the complex reaction into its component simple reactions, measuring the simple reaction times, and then reconstructing the whole complication reaction as the sum of its parts. This additive method set an example for the chronometry of compound reactions which remained in vogue through the first decade of the present century. More recently the additive method of analyzing compound reactions has been discounted, especially by Gestalt psychology, which believes that the "whole" of any act equals more than the sum of its component parts.

The complication experiment has gradually become of less use in psychology because the original astronomical combination of a moving visual stimulus plus a complicating remittant sound has been retained. The eye movements thus introduced tend to invalidate the results. More recently the experiment of comparing a distinct sound with a discrete touch has been made, and has shown that the observer perceives first the stimulus for which he is predisposed. While a complete analysis of the mental processes involved in the complication reaction is still lacking, the effect of attentional predisposition, plus the measurable reaction times, seem to be sufficient to explain the relative personal equations found with the Bradley method.

The simple reaction, as Wundt termed the key-and-chronograph method, has lent itself more readily to psychological experimentation and analysis. The simple reaction was measured first in Wundt's laboratory by recording on a time-registering device the time taken by an observer to react to a signal. These tests established the fact that if the observer was given a ready signal about two seconds before the stimulus, his reaction time was more rapid and more consistent than

without the signal. It was therefore decided, that, in order to secure definitive reaction times which could be reproduced by similar experiments in other laboratories, the warning signal must always be given shortly before the stimulus. This is known as simple reaction with preparatory set, while without the warning signal there is no preparatory set. The key-and-chronograph method of observing transits is really a series of simple reactions with preparatory set, so Wundt's method of testing is analogous to this type of astronomical observation.

It was not until 1888 that Lange showed that there are two types of simple reaction with preparatory set, muscular and sensorial, dependent upon the observer's focus of attention. William James,[32] who was first to interpret the "new" German psychology in America, noted that the reaction time was found to differ in the same person according to the direction of his expectant attention. That is, if the subject thought as little as possible of the movement which he was to make, and concentrated upon the signal to be received, the reaction time was longer; if, however, he concentrated upon the muscular response, it was shorter. Lange found his own "muscular" reaction time to average 0".123, while his "sensorial" reaction time averaged about 0".230.

The difference between the "sensory" and "muscular" types of reaction, dependent upon the focus of attention, as discovered by Lange, has been fully verified and the "muscular" response found to be about 0".1 more rapid. Of this difference Pillsbury[93] says, ". . . the muscles of the arm that is to react are more tense when attention is upon movement than when it is upon response. The tension is more diffuse, it spreads through more muscles in the sensory type of adaptation. Physiologically this means a readiness to react with any muscle rather than with the right hand alone, as in the motor form of reaction." The simple reaction times with preparatory set vary with the individual, *e.g.*, for sound from 0".120 to 0".180, for touch from 0".117 to 0".183, and for sight 0".150 to 0".225.

As stated previously, the key-and-chronograph method of observing meridian transits is essentially a series of simple reactions with preparatory set. Method A, however, seems to be the "sensory" type reaction, while method B is apparently the "muscular" type. In method B, where the observer waits until the wire bisects the star before starting to react, the attention is mainly on movement. It may be expressed by $T + t_m$, where T equals the true transit time and t_m equals the "muscular" reaction time. In method A, (bird on wing), the observer's attention is directed toward the reception of the stimulus, since he must decide how far in advance of the actual transit to start his reaction. It can be expressed by $T - \Delta t + t_s$ where T equals the true transit time, Δt equals the distance in time between the star and the wire when the reaction was commenced, and t_s equals the "sensory" reaction time.

Thus, the reaction time with method B equals t_m while with method

A it equals t_s. The total reaction time is greater for A than for B, but since Δt affects T negatively, compensating for t_s, the apparent personal equation, as shown by star transits, is smaller in Method A than in B. That method A has the larger probable error, may also be illustrated by these formulas. While t_s is greater than t_m, each value seems to be fairly consistent in the same observer. The Δt, however, depends on the judgment of the observer, and is therefore variable. It is this judgment term, absent in method B, which increases the probable error of method A. The variation of the personal equation with declination as found with method A also is due to the inability of the observer to adapt his judgment of Δt to varied speeds of the stars.

To astronomers at the beginning of the present century, the various forms of personal error revealed by experiments seemed to be inherent ills of the key-and-chronograph method of observation. Despite Wundt's discovery that the reaction to sound was shorter than the reaction to touch or to taste, some astronomers went back to the Bradley method in an attempt to cut down personal equation by combinations of stimuli to different senses. John Locke of Cincinnati mentioned, in describing his new chronograph and transmitting clock, that with the Bradley method, if the observer so desired, he could feel the clock "beat," or put the transmitting wire into his mouth and taste it. Several intrepid experimenters actually wired themselves into the clock circuit, but no decrease in personal equation was found.

Other astronomers, however, turned their attention toward the new Repsold hand-driven, traveling-wire micrometer, which was just emerging from the developmental stage. Experiments with this new type micrometer had given promising results in the diminution of personal equation, and astronomers hoped that here, at last, was the foil of personality in transit circle work.

SECTION IV

The Traveling-Wire Micrometer

Before tracing the history of the Repsold or traveling-wire micrometer, it should be noted that there is no distinct boundary between the previous period of key-and-chronograph observation and the present period. The traveling-wire micrometer was successfully adopted at Munich and at Potsdam as early as 1890, but on the other hand, the key-and-chronograph remained in use with the nine-inch transit circle at Washington until 1936. The mean date of the adoption of the Repsold micrometer at Cape, Greenwich, and by the six-inch transit circle at Washington is 1909, but no more definite date can be set for the transition in the mode of observing.

The search for some method of following the motion of a star with the transit instrument started early in the nineteenth century. One idea was to move the entire instrument so that the star remained bisected by a stationary wire in the reticle. Emmanuel Liais suggested this in

a memoir to the Paris Academy of Sciences in 1858, and Repsold later made the same suggestion in 1887. The plan was considered impractical, however, since the mechanism required was too massive.

Another plan was to keep the instrument stationary, and to follow the star transit by means of movable wires in the reticle. The earliest definite references found to something of this nature are to a plan by Wheatstone. At a Royal Astronomical Society meeting in 1864, Warren De La Rue[15] mentioned that Wheatstone's idea was to have the micrometer wires follow the star's motion and make automatic contacts for the chronograph record.

In 1859 Liais[11] gave a detailed plan for a hand-driven, moving-wire micrometer. Contacts on the head of the micrometer screw operated two chronograph pens, one recording hundredths, and the other whole revolutions. Two more chronograph pens recorded seconds and minutes from the standard clock, while a fifth pen, operated by an observing key, indicated which part of the chronograph record the observer wished to use.

An idea for a traveling-wire micrometer driven by a clock-work mechanism which was mounted right on the side of the micrometer was suggested several years later by C. Braun.[67] In 1863 he revised his plan, and proposed in a letter to Airy that the clock-work mechanism be mounted on a pier beside the transit instrument, and that the micrometer be driven by means of rods through one pivot and down through the cube to the micrometer. In this same year, M. Redier[16] at the Paris Observatory devised a micrometer with the eyepiece and one movable wire driven by clock-work. There is no description of the manner in which the chronograph contacts were made, however, or of how the speed of the wire was controlled.

In 1889 Repsold[30] gave a plan which reverted in principle to Liais' idea for a traveling-wire micrometer. Repsold's scheme for an "impersonal" micrometer was to introduce motion to the wire by hand drive. A rod, having knurled wheels at its extremities, extended through the base of the micrometer box. This rod was geared to the screw moving the eyepiece slide, which in turn was connected by a gear train to the screw operating the movable wire. A contact arm attached to the micrometer box and bearing against a break circuit drum on the micrometer screw, automatically made the chronograph record as the wire moved across the field. In using this type of micrometer, the observer, by means of the knurled wheels, turned the driving rod at a speed to conform with the star's motion, thus keeping the movable wire on the star as it crossed the field. Micrometers of this type were tried by Seeliger at Munich and the Geodetic Institute at Potsdam with success.

Gill, in his "*History and Description of the Royal Observatory, Cape of Good Hope,*" gives an interesting discussion of the Repsold micrometer. Gill wanted an impersonal micrometer for the new transit circle

being planned at Cape Observatory in 1890. He seriously considered a method of making a photographic star trail, but, in experimenting with a three-element photo-visual lens, found that the central lens did not react to temperature changes as quickly as the two outer elements. This unequal temperature effect caused an undesirable change in the focal length of the lens, so the scheme was abandoned.

Gill then turned his attention to the traveling-wire micrometer. In 1896 he tried a hand-driven Repsold impersonal micrometer at Repsold's shop in Munich, but felt an undue sense of strain in following the star transits, which he later attributed to lack of practice with this method. He expressed the opinion that it could be improved by some way of driving the wire mechanically. As previously noted, Redier had used a clock drive for a traveling-wire micrometer at Paris in 1863. In 1901 Karl Struve[47] at Königsberg added clock-work driving mechanism to a Repsold micrometer, and in 1902 Cohn[49] discussed some results with this method. In this instance the clock-work and cone to regulate the speed of the wire were mounted right on the micrometer. The wire was set to run slightly slower than the star and the additional impetus was added by the hand drive. Thus, the observer still had to drive by hand to record an accurate transit.

Gill did not like this idea of mounting the clock-work directly on the micrometer, and Repsold stated that "the addition of a clock-work drive was neither necessary nor desirable"[94] in the use of his impersonal micrometer. Repsold advocated, however, that, if astronomers insisted on using some sort of motor drive, C. Braun's scheme of mounting the motor independently of the micrometer and transmitting the driving motion by rods through the pivot and cube of the transit instrument be used. Gill finally settled on an electric motor drive for the new Cape transit circle although Repsold refused to adapt the new micrometer for motor drive until Gill agreed to try the hand drive method as well as the motor.

The micrometer was put into use on the new transit circle in 1905, and the motor drive evolved by Gill was a decided improvement over the previous clock-work drives. It consisted of a constant speed electric motor, a Grubb sun-planet differential gear, and a driving cone mounted beside one pier of the instrument. Rods through the pivot and cube transmitted the motion to the micrometer. Guiding keys, held by the observer, operated the differential gear. The driving mechanism in use with the transit circle at Cape today appears to be essentially the same as Gill's design.

At Greenwich a hand-driven traveling-wire micrometer was adapted to the old transit circle in 1915.[88] More recently a new seven-inch transit circle, designed for motor drive, and closely modeled after the Cape instrument, has been put into use.[101] Since its development by Repsold, the traveling-wire micrometer has also come into use at Washington, Paris, Bergedorf,[78] Trieste,[91] Brussels,[92] and several other prin-

cipal observatories. At Washington it was first used in 1907 on the six-inch transit circle.

In 1929 C. B. Watts[82] of the U. S. Naval Observatory developed a type of motor drive distinctly different from Gill's design. Essentially it consists of a small, light-weight, two-phase synchronous motor mounted directly on the micrometer box, and connected with the gear train driving the micrometer screw. The speed of the moving wire can be varied to observe stars of different declinations by changing the frequency of the power input. Slight corrections for guiding on a star are applied by introducing small changes in the initial frequency of the input current. This type of motor drive has been in use with the six-inch transit circle at Washington since 1933, and with the nine-inch transit circle since 1936.

M. B. Snyder[51] seems to have been the first to seriously consider mounting an electric motor drive directly on the micrometer. In 1902 he gave the following outline of the three types of motor drive available at that time: "I. A small electric motor may be placed on or near the head of the transit instrument, with its axis parallel to that of the instrument. The varying rate of motion required for change in declination may then be secured by regulating the field of the motor and, if necessary, also that of a small dynamo supplying the current. The main difficulties of this plan are the wide range of speed regulation and the interference due to inertia at starting. II. Equatorial speed that is absolutely constant but slightly regulable may be given the motor, similarly placed, and the differing rate of motion proper to each declination determined by mechanical gearing, consisting principally of two friction disks placed at right angles to each other, or by some other mechanical equivalent . . . III. It may in some instances be desirable to place the electric motor on a separate support near the base of the instrument, and then by means of a light steel shaft entering the axis of the transit finally communicate the required motion to the micrometer screw . . ." Snyder constructed a motor drive of the second type mentioned above which was used on a small transit instrument at Philadelphia in 1902. Interest in the project must have waned, however, as there is no further mention made of it. The type of motor drive suggested in plan I did not become feasible until the later development of a suitable synchronous motor.

The inherent ills of the key-and-chronograph method of observing transits had been the variation of personal equation with the declination, magnitude, and direction of motion of the star. With the advent of the traveling-wire micrometer astronomers were interested to see if these personality factors had been diminished or eliminated. The first encouraging word came from Gill and Hough[56] at Cape. They conducted tests for variation of personal equation with star magnitude, using the old transit circle with the key-and-chronograph, and the new transit circle with the hand-driven traveling-wire micrometer. The

magnitude personality was very evident in the results of the old instrument, but no trace of this error could be found in the observations with the impersonal micrometer. Again, J. C. Hammond[71] found no evidence of a magnitude equation between fifth and eighth magnitude in observations with a hand-driven impersonal micrometer on the six-inch transit circle at Washington. R. M. Stewart[61] also found no measurable magnitude equation evident in observations with an impersonal micrometer.

The variation of personal equation with declination in key-and-chronograph observations as caused by the use of observing method A has been previously discussed. This variation entered because the observer attempted to compensate for his reaction time in pressing the key by anticipating the moment the star would cross the micrometer wire. With the traveling-wire micrometer, however, the star is kept continuously bisected as it crosses the micrometer field. The observation then becomes merely a comparison of the position of the object and of the micrometer wire traveling at approximately the same speed. This eliminates the necessity of anticipating the bisection of a moving object with a stationary wire, and thus the variation of personal equation with declination due to this form of anticipation disappears.

An observation with a traveling-wire micrometer may be described as a series of complex reactions with preparatory set. The preparatory set in this case is of the sensorial type; that is, a general readiness on the part of the observer to react in one of two ways. The observer, in guiding, must discriminate which way the star has drifted, and must correct the traveling wire accordingly. In this type of observation, where the observer must discriminate between two stimuli and their respective responses, the reaction times are more variable and on the whole longer than in the so-called simple reaction. With the key-and-chronograph method the observer's reaction time entered the right ascension determination with full weight. In impersonal micrometer observations, however, the reaction time of the observer affects the right ascension determination very slightly, if at all, since the micrometer wires and the star are moving at approximately the same speed. Thus, while in observations with the impersonal micrometer the personal reaction time may be actually greater than in key-and-chronograph observations, its influence on the observed right ascensions is markedly less.

In conjunction with the impersonal micrometer, two methods of registering the position of the movable wire have developed. The earlier type consists of a contact drum connected to the micrometer screw which sends signals to a chronograph at known positions of the traveling wire.[64] More recently, Watts[115] has developed the photographic register, which photographs the micrometer head at four-second intervals as determined by the standard clock. The cardinal difference between the two methods is the time needed for a complete

transit observation and the resultant fatigue of the observer. With the contact drum the time required for a complete observation depends on the speed at which the traveling wire is moving, and thus varies with the declination. An equatorial observation may take only thirty seconds, while a near-pole transit will require four or five minutes. With the photographic register, six photographs of the micrometer head, at four-second intervals compose the transit observation; thus it takes no longer to observe a circumpolar star than an equatorial star.

Aside from the time-saving advantage in observing stars of high declination, the photographic register may also offer a definite increase of accuracy in these observations. As mentioned previously, an observation with the impersonal micrometer is a series of reactions with preparatory set. This preparatory set, however, cannot be sustained over very long periods, since muscular and neural fatigue develop, inducing the observer to adopt a less attentional attitude. Thus in observations of near-pole stars with the contact-drum method, the observer is extremely liable to become fatigued and lose his attitude of preparatory set; while with the photographic-head method the observer is able to retain his state of preparatory set while observing near-pole stars as completely as with equatorial stars.

It is common practice in using contact-drum registration to give the observer an audible signal after each drum contact, so that during observations of near-pole stars the observer can relax between signals and thus avoid extensive fatigue. After each succeeding period of relaxation during the observation, however, it does not seem probable that the degree of preparatory set attained by the observer would be as complete as that reached by an observer making a continuous and complete observation in a short period. Another method to avoid fatigue in near-pole observations with contact-drum registration is used at both Cape[66] and Greenwich. The micrometer wire is set to move at equatorial speed and allowed to travel back and forth across the star image; with each coincidence of star and wire the observer operates a punch key. An objection to this method might be, however, that the azimuth stars are not observed in the same manner as the other stars of the program, with a resultant variance in personal equation.

With the increased accuracy of observation afforded by the impersonal micrometer, a new personal error came to light; namely, leading error. In using the Repsold micrometer the observer keeps the movable wire on the star by turning the hand-driven micrometer screw. Thus the observer always has a tactual reminder that the wire and the star are in motion. Also, the fixed wires of the micrometer field drifting past the movable wire and star give a visual impression of relative motion. These reminders that the wire and star are in motion destroy the illusion that the star is being bisected at rest. Thus the observer tends to drive the wire a little ahead, or slightly behind, the center of the star. Leading error should not be confused with bisection error,

since the former is present only when the observer is conscious of the motion of the star and wire, and does not exist in measures made at rest. Also the leading error is not eliminated by the reversing prism, as is bisection error, for although the prism reverses the apparent direction of motion of the star, the micrometer screw continues to turn in the same direction. R. M. Stewart[61] found evidence of this leading error with the Repsold micrometer, and M. N. Stoyko[84] has given a mathematical expression of its effect on observed transits. Since the known personal errors evident in the key-and-chronograph method are theoretically eliminated in the Repsold micrometer, the personal equations found with this type of micrometer should be composed mainly of the leading error. This is on the assumption that the bisection error is negated through use of the reversing prism.

The personal equations found with the hand-driven traveling-wire micrometer of the six-inch transit circle in 1930 for five observers were $+0^s.012$, $+0^s.004$, $0^s.000$, $+0^s.015$, and $+0^s.010$.[96] The values are noticeably smaller and much more accordant than the personal equations detected in previous methods of observing.

With the perfection of motor-drive methods for the Repsold micrometer, the leading error has been greatly reduced. The observer no longer has the tactual reminder that the star and wire are in motion, and thus the illusion of bisection at rest is more nearly complete. The motor drive has also reduced the accidental errors of the observation. Thus the personal equation found with the motor-driven Repsold micrometers today are much smaller, more accordant, and more nearly constant for each observer than were those noted with the key-and-chronograph method. For example, the absolute personal equations determined in the current program of the nine-inch transit circle run from $+0^s.001$ to $-0^s.004$, $\pm 0^s.002$, while the relative personal equations determined recently on the six-inch transit circle vary from $-0^s.004$ to $+0^s.005$, $\pm 0^s.002$. The greater range in the latter example may be attributed to the fact that these relative personal equations include the errors in determining the instrumental constants in addition to the errors of observation.

It seems highly probable that the small leading error, evinced by the above personal equations as still evident in the motor-driven Repsold micrometer method, is due mainly to the fixed wires in the micrometer field. These wires apparently moving past the star and movable wire give a visual impression of relative motion which hampers the bisection-at-rest illusion. At present the leading error, and the personal equations, are smaller than the other errors of the transit observations. In the future, when the accuracy of transit observations has been increased to the point where the leading error becomes appreciable, the fixed wires in the micrometer field might be removed, thus completing the bisection-at-rest illusion. The main function of the fixed vertical wires is to indicate to the observer the position of the traveling wire in the

field. This same function might be performed by audible signals, which would not give the visual impression of relative motion that the fixed wires do. This change should eliminate all indication that the star and traveling wire are in motion, except perhaps for a slight appreciation of muscular sensation as the observer moves his head to accommodate for the motion of the eyepiece slide. The leading error seems to vary approximately as sec δ. Near the pole, however, at that declination where the observer is no longer conscious of diurnal motion, the leading error ceases to exist. This declination, dependent upon the individual's discrimination threshold, probably varies to some extent among observers.

The bisection error, which was first noticed in key-and-chronograph observations, continued to be present in the traveling-wire method. D. B. Nugent[62] found bisection errors of $0^s.0016$ and $0^s.0260$ in two observers but discovered no correlation between bisection error and the magnitude or declination of the star observed. Other astronomers have also established the presence of bisection error and in the last decade a number of observatories have adopted the reversing prism to eliminate this error. A series of transits observed by one man, without reversing prism, will be affected systematically by the amount of his bisection error. Even if he makes his own determination of the instrumental constants by which these observations are to be reduced, a partial effect of the bisection error will still be present. In bringing together the work of several observers, done without reversing prism, the residual of their combined bisection errors will appear as a systematic error in the resultant star positions, for in combining the results of several observers there is no assurance that their bisection errors will cancel out. According to R. S. Woodworth,[75] in an experiment on judging the center of an interval, the mean estimate of ten subjects in the horizontal coördinate was 0.509, and in the vertical coördinate 0.486.

The psychological reason behind the bisection error is not clear. The nativistic and empiristic schools are at odds as to the manner of development of our space perception. But whether inborn or learned by experience, both schools agree that there is a definite variance in judgment among individuals in estimating the mid-point of an interval. This improper judgment, which becomes habit through usage, seems more correct to each individual than does the true center of the interval as determined by an impersonal machine. One solution to bisection error might be remedial training of judgment for each observer, but a much simpler expedient is the use of the reversing prism.

The reversing prism, however, is not a remedy for all instances of bisection error. To be fully effective in use with the Repsold micrometer having double traveling wires, the observed object must appear symmetrical when centered between the movable wires. The stars satisfy this requirement suitably, but some of the planets, and more notably the sun and moon, do not, since their apparent diameters greatly

exceed the angular separation of the movable wires. In these cases the bisections are made at the visible limbs of the object, and it seems to be in these instances that the reversing prism fails to eliminate bisection error in its entirety.

Although there has been extremely little experimentation on this point, it seems highly probable that in centering an arc between the movable wires an observer's bisection error for a right-hand arc, *i.e.*, extremities toward the right, will differ from that for a left-hand arc. This statement applies also to limb declination observations. There is no implication that this difference is constant; its amount undoubtedly varies among individuals. In observations of the sun where limbs I and II are observed both direct and reversed, the residual of the bisection errors for right-hand and left-hand arcs would not affect the observed position of the sun but would appear as a change in the observed diameter of the sun. If a reversing prism is not used in the observation, the residual bisection error would have the effect of shifting the observed position of the sun. In the case of the moon and of the planets where often only one limb is visible, the residual bisection error of observations made with a reversing prism would enter directly the observed position of the object.

This seems to be the case with observations of Mercury made during the latter part of the nineteenth century; and the introduction of more accurate observing methods in the present century, rather than eliminating this effect, has tended to delineate it more clearly. As noted by H. R. Morgan,[107] personal errors of observation tend to average out in a synodic period. Thus the residual bisection error might average out of the observed position of the object in a synodic interval, but it would still affect the observed diameter of the object. If this residual bisection error exists, then in a synodic period it should be revealed as a difference between the observed diameter and the true diameter of the object; and this difference seems to be confirmed in meridian transit observations of Mercury.[110] These observations of Mercury are complicated somewhat by the fact that the center of light is usually observed instead of the limb. Since the visible figure of the planet appears unsymmetrical, except when very near superior conjunction, it seems reasonable to assume that this residual bisection error is present here as well as in limb observations. Another indication of residual bisection error seems present in observations of the moon with the six-inch transit circle at Washington. In combining, by observer, the observations in each synodic period, the resultant semi-diameters have been found to differ systematically. The use of a single movable wire to eliminate residual bisection error in limb observations might be suggested. The inaccuracies introduced in attempting to keep an opaque micrometer wire tangent to a luminous limb might prove greater than the amount of the residual bisection error, but would probably be accidental, rather than systematic.

There are two other forms of bisection error not previously mentioned. One form is that resulting from unsymmetrical star coma. This unequal flaring of light, especially noticeable on brighter stars, is a result of the optical properties of the objective. The presence of coma often induces the observer to bisect the center of illumination, rather than the star itself. This bisection error cancels out of the final position of the star, however, if the instrument, and hence the objective, are reversed at frequent intervals. Again, in observations near the horizon, the star images are elongated in the declination coördinate due to atmospheric dispersion. In effect the images become tiny spectra of the star light. Due to personal differences of color sensitivity, individual observers may bisect the elongated image at different points. Reversal of the instrument does not lessen this effect.

The use of the reversing prism in an observational program is important in determining an accurate relative system of stars. Using modern transit circle methods, the probable error of a single observation as determined at Washington, is $\pm 0^s.011$ in right ascension. Therefore, with approximately thirty observations of each star, it is possible to establish a relative system of stars accurate (with regard to accidental errors) to $0^s.002$ in right ascension. These stars, however, cannot be put on a system of fundamental coördinates with this same degree of accuracy, since the equinox can be determined with only about one-fifth of the accuracy with which the relative star positions can be established.

The astronomers of today are reaping a grim harvest, in fundamental and definitive catalogues, of the personal errors sown in the observational catalogues of the nineteenth century. The variation of right ascension determinations with the declination has been previously noted as a defect of method A in key-and-chronograph observations. This error, which is evident in many of the older observational catalogues, has been reflected in the definitive systems based on these catalogues. In another instance, the corrections for variation of right ascension with star magnitude applied to these old observational catalogues in forming a definitive catalogue have been incorrect, with the result that in the final definitive system the bright stars are rotated with respect to the faint ones.

The personal errors contained in the old observational catalogues also may affect the derived proper motions. In determining accurate proper motions it is desirable to have a relatively long period of time between the end catalogues. H. R. Morgan[107] has pointed out that it is impossible to get very reliable proper motions in ten or twenty years, since the errors of the end positions are too large. Thus in deriving our present proper motions the primary end catalogues were mainly those of the nineteenth century. The best determined of these proper motions have been used to form the basis of certain statistical relations, which have been applied to the great mass of stars. Therefore, if the errors

of the primary end catalogues have been reflected in the derived proper motions, the statistical conclusions based on these proper motions are subject to error.

The positional astronomers of today, with these and other factors in mind, are striving to avoid or at least negate the personal errors of observation. The results are already apparent in the form of more accurate standard observational catalogues. The two fundamental catalogues used at present are the Third Fundamental Catalogue of the *Berliner Astronomisches Jahrbuch* and the *Albany General Catalogue*. However, both of these catalogues are becoming out-dated; the mean epoch of their observational data is about 1900. It is hoped that within the next two decades a new fundamental catalogue can be compiled, based mainly on the standard observational work of the present century. With the increased exactness of observation afforded by modern transit circle methods, a fundamental star system and the accompanying proper motions determined from the work of this period should show a greater degree of systematic accuracy than has been previously attained.

(To be continued)

More Experimental Moon Craters

By R. D. COOKE

A recently published article in POPULAR ASTRONOMY* sought to illustrate the formation of model lunar craters by dropping powdered cement onto a bed of the same material.

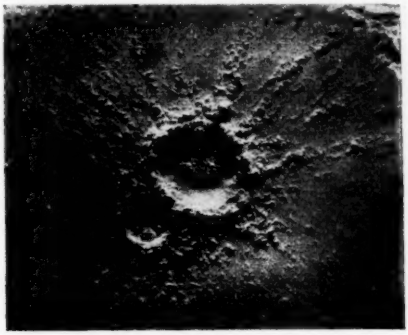
The article referred to recalled some observations along similar lines made by the writer a number of years ago and heretofore unpublished. These models were on a very much smaller scale than those of Ley, being from two to twenty-five mm in diameter, but they illustrate more diversity of lunar characteristics and to a certain extent more clearly. For these reasons it is deemed advisable to publish them at this time while the subject is receiving attention.

The effect was discovered quite by accident, and it appears in the light of later experiments that a powder of very special peculiarities is required to achieve it. Attempts to duplicate the effect with a number of other powdered materials including flour, pumice, plaster of Paris, whiting, cement, and clay have met with conspicuous lack of success.

The powder used was dry process porcelain enamel powder, an intermediate raw material used for the enameling of bath tubs, etc., and would seldom be encountered by anyone outside of that industry. Its composition resembles that of glass, a complex silicate, and has about the same specific gravity as glass. The fineness was such that nearly

*Willy Ley, Experimental Moon Craters, POPULAR ASTRONOMY, **52**, 278, June, 1944.

all passed 100 mesh and 20 per cent to 30 per cent was retained on 200 mesh standard screens. The fineness distribution tapered down to particles of colloidal size. The best results were obtained with the powder slightly dampened,—about 0.5 per cent moisture.



ARTIFICAL CRATERS

The original photographs from which the accompanying reproductions were made were full size, but are here reduced to one-half linear scale. The powder was spread about one-half inch deep and smoothed with a spatula. Bits of the same powder picked up on the point of a knife were dropped from heights of ten to twenty inches. The various types of splashes illustrate almost every form of crater seen on the moon, including overlapping craters, central peaks, craters within craters, and the rays radiating from Copernicus and Tycho.

ARTIFICAL CRATERS

The reader may be troubled by the phenomenon of optical inversion when viewing these pictures. That is to say hills appear as depressions and *vice versa*. It will be helpful to let the light fall on the page from different directions until the image rights itself in the mind.

ARTIFICAL CRATERS

The writer disclaims any implication that this constitutes a "proof" of the impact theory of lunar craters, but the analogy is too striking to be ignored in any discussion of the formation of lunar features. The impact theory has long been in disrepute but has acquired some respectability in recent years. It is believed the present contribution

ARTIFICAL CRATERS

strengthens the arguments in its favor. It may be pointed out that while previous discussions have assumed the surface of the moon to be solid rock, this need not be the case. Undisturbed as it is by atmospheric conditions and subject to a low gravitational force, the surface to a considerable depth could as well be regarded as of a loose stony to granular structure without violating any of the known facts about the lunar landscape.[1]

[1] A substantial part of the expense of reproducing the accompanying photographs has been generousiy unwritten by the Joliet Astronomical Society.

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The Planets in March, 1945

Sun. The sun will move northeastward from $\alpha = 22^h\,46^m$, $\delta = -7°\,48'$ to $\alpha = 0^h\,36^m$, $\delta = +3°\,56'$. It will move from Aquarius into Pisces. Twenty-two minutes before midnight at Greenwich on March 20, the sun will cross the equator, and thus mark, according to the calendar, the beginning of spring.

Moon. The phases of the moon will occur as follows:

	d	h
Last Quarter	March 7	4
New Moon	14	4
First Quarter	20	19
Full Moon	28	18

It will be farthest from the earth on March 2 and nearest the earth on March 15 during this period.

Mercury. Mercury, having been at superior conjunction on February 28, will move eastward more rapidly than the sun. It will continue to do so until March 26 when it will be at greatest elongation east of the sun. At that time it will be about ten degrees north of the sun which will delay its setting after the sun and conditions will then be favorable for seeing this rather elusive object. It will then be quite near the fourth magnitude star o Piscium, and, of course, much brighter than it.

Venus. Venus will still be well east of the sun and hence will continue to be the brilliant evening star. It will, however, be lower in the sky from evening to evening at sunset. A period of greatest brilliancy will occur on March 10 when it attains magnitude —4.3. At the close of the month Venus will be in the western part of Aries, in a region devoid of bright stars.

Mars. Mars will be moving eastward a trifle more slowly than the sun. At the end of the month it will be a little more than two hours west of the sun. It will be on the average about fifteen degrees south of the equator, which will be an unfavorable circumstance for northern observers. During the month, Mars will move from Capricornus to Aquarius.

Jupiter. Jupiter will continue a slow westward motion among the stars. It will be in opposition to the sun on March 13, and will consequently be above the horizon all night. It will be about midway between Regulus and Spica.

Saturn. Saturn will be moving westward at the beginning of the month but will become stationary and begin moving eastward on March 6. On March 25 it will be in quadrature, 90° east of the sun. At this time it will be on the meridian at sunset and hence be well situated for evening observations.

Uranus. Uranus will be a little less than two hours west of Saturn, and in practically the same declination as Saturn. It will be Taurus a short distance northeast of Aldebaran.

Neptune. Neptune will be in opposition to the sun on March 26. It will move westward slowly, almost exactly on the equator. Its position on March 15 is $\alpha = 12^h\,22^m$, $\delta = -0°\,46'$.

Pluto. The position of Pluto, as given by the Nautical Almanac Office, for March 14 is $\alpha = 8^h\,49^m.7$, $\delta = +24°\,03'$.

Asteroid Notes

By HUGH S. RICE

We have been observing asteroids Juno, Ceres, and Pallas with Zeiss 50-mm. Vesta and Iris are also getting to be within range of small instruments, so that during the period of about February 15 to March 15, 5 minor planets are available.

Juno is in Orion headed toward α Orionis, on the way to completing a retrograde loop. On February 24 it completes the loop, intersecting its former apparent path of September 29. About 0^h U.T. February 20 it is in conjunction with the star. However it makes its closest approach about 0^h February 19, when the asteroid is 0°.3 northwest of α. With a clear sky, observers should be able to pick up the planet whose magnitude at the time is expected to be about 8.6. It is moving in a northeasterly direction.

The following tabulation summarizes the approximate locations and estimated visual magnitudes on March 1, and the date of opposition of the planets mentioned.

Planet	Place	Mag.	Opposition
3 Juno	α Ori	8.9	Dec. 24
7 Iris	φ Leo	9.0	Mar. 6
2 Pallas	γ Cor	6.5	Mar. 22
4 Vesta	ξ Vir	7.3	Apr. 9
1 Ceres	φ Vir	8.2	Apr. 26

The following ephemerides are given us by the Yale University Observatory.

EPHEMERIDES OF ASTEROIDS, 0^h U.T.

3 JUNO

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Feb. 14	5 48.5	+ 6 47	Mar. 6	6 3.0	+ 9 55
19	5 51.1	+ 7 37	11	6 8.0	+10 36
24	5 54.5	+ 8 25	16	6 13.6	+11 14
Mar. 1	5 58.4	+ 9 11	21	6 19.6	+11 50

7 IRIS

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Feb. 14	11 23.9	— 5 20	Mar. 6	11 5.7	— 3 49
19	11 19.8	— 5 4	11	11 0.9	— 3 18
24	11 15.3	— 4 43	16	10 56.2	— 2 46
Mar. 1	11 10.6	— 4 18	21	10 51.9	— 2 13

2 PALLAS

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Feb. 14	12 22.5	— 5 46	Mar. 6	12 15.1	+ 1 23
19	12 21.5	— 4 11	11	12 12.0	+ 3 24
24	12 20.0	— 2 26	16	12 8.6	+ 5 25
Mar. 1	12 17.8	— 0 34	21	12 5.0	+ 7 25

4 VESTA

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Feb. 14	13 36.3	+ 0 42	Mar. 6	13 37.6	+ 2 13
19	13 37.8	+ 0 59	11	13 36.0	+ 2 45
24	13 38.5	+ 1 20	16	13 33.7	+ 3 19
Mar. 1	13 38.4	+ 1 45	21	13 30.7	+ 3 54

1 CERES

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Feb. 14	14 33.5	— 2 36	Mar. 6	14 39.4	— 2 9
19	14 35.9	— 2 33	11	14 39.3	— 1 56
24	14 37.7	— 2 28	16	14 38.5	— 1 42
Mar. 1	14 38.9	— 2 20	21	14 37.0	— 1 26

Hayden Planetarium, American Museum of Natural History, New York, N. Y., January 21, 1945.

Occultation Predictions for March, 1945

(Taken from the *American Ephemeris*)

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

			IMMERSION				EMERSION			
Date 1945	Star	Mag.	Greenwich C.T.	*a*	*b*	Angle E from N	Greenwich C.T.	*a*	*b*	Angle E from N
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
Mar. 9	191 B.Sgtr	6.5	8 43.6	—0.6	+1.0	102	9 53.7	—1.3	+1.1	270
19	68 Taur	4.2	3 32.4	+0.1	—1.4	96	4 24.7	+0.2	—0.6	249
22	δ Gemi	3.5	4 59.9	—0.9	—0.5	60	5 46.3	+0.4	—2.4	321
OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′										
Mar. 19	68 Taur	4.2	3 36.0	—0.2	—2.4	121	4 22.6	—0.5	+0.3	223
22	δ Gemi	3.5	4 43.8	—1.2	—1.2	90	5 52.0	—0.4	—1.8	291
OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′										
Mar. 10	4 Capr	6.0	13 0.1	—0.8	+1.1	91	14 11.4	—1.5	+1.1	265
17	389 B.Ceti	6.3	2 38.5	—0.9	—0.4	71	3 41.7	—0.5	—0.4	247
22	δ Gemi	3.5	4 6.3	—1.9	—2.6	132	5 19.1	—2.3	+0.5	245
23	49 B.Canc	5.9	7 25.9	—2.0	+0.1	62	8 16.3	+0.1	—3.2	334
OCCULTATIONS VISIBLE IN LONGITUDE +98° 0′, LATITUDE +30° 0′*										
Mar. 17	389 B.Ceti	6.3	2 55.9	—0.2	—1.0	91	3 47.5	0.0	+0.2	234
19	BD+17°724	6.9	4 25.3	—0.2	—1.6	109	.. ..	..	..	...
22	δ Gemi	3.5	4 53.5	—0.9	—2.5	130	5 59.0	—1.2	—0.5	255
22	149 B.Gemi	6.5	8 28.6	+1.3	—3.3	164	8 52.7	—0.4	+1.1	218
23	BD+21°1792	6.8	4 53.3	..	..	170	.. ..	..	..	...
23	49 B.Canc	5.9	8 03.7	—2.8	+3.7	31	8 19.0	..	..	2
24	80 Canc	6.8	7 51.8	+0.1	—2.8	152	.. ..	..	..	...
30	566 B.Virg	6.5	3 21.1	—1.4	+3.1	63	4 01.2	—0.1	—2.5	348

*Computation by George R. Schaefer and Paul Herget, communicated by Commodore J. F. Hellweg, Superintendent U.S. Naval Observatory.

METEORS AND METEORITES

Contributions of the Society for Research on Meteorites

Edited by FREDERICK C. LEONARD
Department of Astronomy, University of California, Los Angeles 24

Directions for the Etching and Preservation of Metallic Meteorites*

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Colorado Museum of Natural History and American Meteorite Laboratory, Denver

INTRODUCTION

Whether intended for museum exhibition or for study, the manner of etching a metallic meteorite specimen has much to do with its value. Faulty technique may result in discoloration, distorted patterns, indistinct figures, or a protracted chemical action which may permanently injure the specimen or in some cases may in time accomplish its complete disintegration.

The following instructions are based upon 13 years of experience, during which time more than 1000 specimens have been treated, including all of the well-known groups of metallic meteorites and involving about 150 different falls. The writer lays no claim to complete knowledge on the subject, but speaks merely as a practical worker in the field and as one who has encountered many difficulties, some of which have been successfully overcome. No amount of instruction will render one proficient in the art of etching. Only by experience will one perfect his technique. Any reader of this paper who aspires to master the handling of meteorite specimens will do well to follow the reading by experimenting with a few of the less expensive metallic meteorites.

MATERIALS NEEDED

Elaborate equipment is not necessary for the securing of excellent results. The following materials are ample: (1) etching solutions; (2) a shallow glass or porcelain dish, of a size suitable for the specimens to be treated; (3) a soft, bristle brush, 1 inch wide—the etching brush; (4) 2 small camel's-hair brushes of good quality, one sharply pointed; (5) a clean, soft towel, as free from lint as possible; (6) running water; (7) good white shellac and a brush; (8) some good-grade, clear-brushing lacquer; (9) lacquer thinner; (10) a reading glass; (11) grain alcohol.

For the majority of meteorites, the best etching solution is a 6% solution of HNO_3, prepared by placing 30 cc. of concentrated HNO_3 (C.P.) in a container and adding distilled water to make 500 cc. of solution. If this solution works too slowly, it may be enriched by adding more acid, or, if too strong, it may be diluted. For the majority of specimens it will be of the correct strength. For

*[This very useful paper, which, in its original form, appears to be out of print, is here reprinted, with the permission of the author and the Director of the Colorado Museum of Natural History, from *Proc. Colorado Mus. Nat. Hist.*, **15**, No. 1, 3-8, Feb. 1, 1936. The 8 illustrations that accompany the paper, *loc. cit.*, are omitted.—ED. *C.S.R.M.*]

the average octahedrite, the 6% solution will render the Widmanstätten figures visible within 5 or 10 seconds, and, to complete the process, only 2 to 5 minutes should be necessary.

If the solution is too weak, the longer time necessary for the etching process allows the acid to penetrate deeply between the plates. This acid will be difficult to remove and, if allowed to remain, will later cause undesirable stains and may lead to a slow but persistent disintegration. On the other hand, if the solution is too strong, it will be difficult or impossible to bring out the delicate details of pattern. The etching pattern is due to the different degrees of solubility of the 3 alloys, *kamacite, taenite,* and *plessite,* together with such inclusions as may be present. Strong acid will attack all of them readily and therefore will not reveal sharp delineations between them. Also, the stronger solutions work too fast to allow the operator to check the action at precisely the right instant for the best results. Etching meteorites is very much the same as the development of the photographic plate. Some sort of picture may be obtained, even tho very crude methods are used, but the perfect result is obtained only by precisely the correct treatment. This must be learned by experience.

Preparing Specimens for Etching

All specimens to be etched should be ground to a plane surface on coarse carborundum or emery, then ground on successively finer grits until they are finished on a grit of 400 or finer. For exhibition purposes, the 400 grit is entirely satisfactory. For research purposes, especially in connection with hexahedrites and ataxites, specimens should be finished on felt with tin oxide, aluminum oxide, rouge, or some other, equally fine-grained, polishing material.[1]

When prepared as thus indicated, examine each specimen for cracks or fissures. If any are present, fill carefully with dilute shellac (dilute ordinary shellac with alcohol; about 3 parts of alcohol to 1 of shellac). Also, give the edges and back side of the slices a generous coat of shellac. Special care should be exercised to see that all fissures bordering the polished surface are flooded with shellac. Allow the specimens to dry thoroly. If, in filling the crevices, some of the shellac has been accidentally spread onto the polished metal, the specimen may, after drying, be again polished off briefly on the 400 grit. This procedure will not remove the shellac from the crevices and will leave the polished metal free for the etching.

Now examine the surface for sulfide inclusions. If any are present, cover them carefully with a good grade of clear-brushing lacquer, using a small pointed brush to fill in the irregularities and to cover the junction of the nodule with the surrounding metal without covering any of the metal. Here the reading glass will prove to be a valuable aid. It is very important that the edges of the nodules be covered, because the least exposure will produce an ugly stain if the nodule consists of the soluble variety of sulfide. Allow the specimens to dry. They are now ready to be etched.

The Etching Process

Pour 6% HNO_3 into the glass or porcelain dish to a depth of about ½ cm. You should now be within easy reach of running water. Take a specimen in your hand,[2] polished side up, and, holding it over the acid dish, dip the etching brush into the acid and spread it quickly and evenly over the polished surface. Keep dipping and spreading, never allowing the acid to remain at rest on any part of the slice longer than is necessary. If specimens are of suitable size and shape,

they may be dipped face downward into the dish and then turned up and brushed. *Never allow etching specimens to lie submerged in acid unless all of the unpolished surfaces have been well coated with lacquer or shellac; and never allow acid to remain, unagitated, on the etching surface of any specimen.*

As soon as the figures appear distinctly, or, if the meteorite is not an octahedrite, as soon as the luster has gone from the surface, wash the specimen and dry it quickly with the towel or in an air blast. Blot with the towel. Do not rub. When dry, examine the specimen for any brown or other stains, however small. If any are present, lay it up for thoro drying and treat the remainder of your specimens as the first.

When all the sections are completely dry, go over them with a reading glass and find the sources of all stains—perhaps minute inclusions of sulfide—and cover carefully with lacquer. When dry, return to the etching, and, with a fresh supply of acid, brush away all stains and carry the etching process to the greatest possible brilliance. Remember that it is just as easy to go too far as it is to stop short of the best. The time necessary for the best results will vary with different meteorites; experiment, watching carefully until you learn to recognize the optimum brilliance! If more stains appear during this treatment, go over them again with the reading glass and lacquer any remaining sources of stains. Repeat this process until you have the result you seek.[3]

When satisfied, dry carefully and thoroly. You may warm to hasten drying, but do not heat much above body temperature. An electric fan is very desirable for drying. Do not touch the etched surface with the fingers. A finger mark may demand another treatment for its removal. This fact is especially important in preparing ataxites and some hexahedrites. It is usually safest to lay the prepared specimens back in a dry place, protected from dust, for a day or two, before lacquering, unless you are working in a moist climate, in which case it is best to dry as thoroly as possible and lacquer immediately. Alcohol may be used as a drying agent, where no nodules have been present; otherwise, it may discolor or dissolve the lacquer with which these have been covered, thus producing an unsightly blemish.

Lacquering Specimens

The lacquering is done to protect against moisture and oxygen. The lacquer may be applied either by brush or as a spray, in a uniform layer of sufficient thickness to produce a smooth glaze. Care must be exercised so as not to get it on too thick, in which case it will produce a crinkly surface when dry. Before applying the lacquer, the surface to be treated should be carefully cleaned of any minute lint particles, which are continually settling from the air. These may be swept away by the use of a clean, camel's-hair brush. When specimens are deeply etched (which is not usually advisable), there are likely to be many small projecting points of metal, not visible, but quite bothersome in catching towel lint and holding it, in some cases even in spite of one's efforts to dislodge it by means of a brush. Such lint may be removed by singeing in a smokeless flame of alcohol or gas. The drying lacquer also should be protected, either by laying the specimens in a cabinet or by improvising a shelter of cardboard or other material supported not more than a few inches above the lacquered surfaces. The treated surface should be kept level while drying and should not be touched.

Miscellaneous Suggestions

There are many etching solutions: bichloride of mercury, bromine water,

picric acid, hydrochloric acid, and various others. None of these has proved as satisfactory, in the writer's experience, as has nitric acid. There is, however, one other etching agent which is superior to HNO_3 for certain meteorites that contain inclusions of soluble sulfide. These troublesome inclusions are usually quite small—some of them too small to be seen without a lens. In such cases the infested areas immediately etch to a dull brown color, and a strong sulfurous smell is given off during the etching. Obviously it is impossible to lacquer these minute inclusions without covering the entire surface. In such cases, nitol is desirable to use. Nitol is prepared by mixing 55 cc. of concentrated HNO_3 with 1000 cc. of 95% alcohol. This solution does not work as fast as the 6% nitric-acid solution mentioned previously, nor does it produce quite as brilliant figures; however, it does not attack the sulfides to any great extent and, in most cases, it can be used without producing any stains on the specimens. In a few cases the included sulfide is too soluble to resist even the nitol. In such cases the only recourse is to cover the inclusions as directed before and then to use the HNO_3 as an etching agent. The nitol would attack the lacquer or shellac covering; hence, it cannot be used in such a procedure. Nitol stains the skin of the hands quite readily and should be applied with this fact in mind, whereas the plain nitric-acid solution recommended will not injure the skin unless this is exposed to it for a considerable time.

Whatever the etching agent used, always be careful to see that the skin of the hands is not broken at any point, as the acid may produce serious irritation thru any abrasion. Rubber gloves will protect the hands in such cases.

It has been the practice of some to produce a border around the edge of etched specimens by covering such a border with lacquer or shellac before etching. In the opinion of the writer, this is not a good practice. It gives specimens an artificial appearance and leads to misconceptions in the minds of observers, as has been revealed frequently by the comments of visitors to collections where such specimens are exhibited.

If considered advisable, the name [or, better, the coördinate number—Ed.] of the meteorite can be neatly written on the etched surface by lettering it with lacquer before etching. This practice is of questionable value.

Care of Specimens

Their rarity and consequent value make it important that meteorites be given the best possible care. If specimens are to be much handled, it must be expected that refinishing will be necessary at intervals. The protecting lacquer will become scratched or rubbed off, and exposure to oxygen and the moisture of the hands will soon bring about tarnishing or rusting. A blow, such as is occasioned by the dropping of a specimen onto the floor, appears sometimes to initiate a chemical action which soon registers itself in the form of rust appearing first between the plates of kamacite and later spreading over the adjoining surface.

Meteorites often contain small amounts of the proto-chloride of iron which constitutes a continual threat of damage or destruction to the specimen containing it, depending upon how much of the substance is present and how well the specimen is cared for, as to how serious is its action. A few meteorites have defied all efforts to preserve them against this pernicious offender. The directions given here have proved effective in most cases.

Those who exhibit meteorites will do well to take precautions against subjecting specimens to considerable, frequent, and abrupt changes of temperature.

A museum case of specimens which was so located that it received direct sunlight during a part of the day and only diffused light during the remainder of the time developed many blotches of rust within a period of about 12 months. The same specimens when refinished and treated by the same methods, except that they were moved to a different part of the room, have not shown any of this rusting in 4 years.

Where specimens are exhibited by artificial light, care should be used to prevent periodic changes in temperature thru turning the lights on and off. Filtering the light thru a heat-absorbing medium has been suggested. Ventilation of the cases will render the temperature changes less abrupt.

NOTES

[1] It is very important that the grinding be done carefully with regard to the elimination, during each stage of the grinding, of all scratches or pits produced by the preceding stage. If care has not been exercised on this point, the specimen may appear to be well polished when, in reality, it is merely superficially burnished. In such cases, when the etching solution is applied, there will develop dull, dark patches with indistinct pattern, appearing as if the crystallization had not been complete. No amount of etching will eliminate this defect. The only remedy is to regrind and repolish.

[2] This dilution of HNO_3 is not injurious to the normal skin of the hands unless it is kept in contact for a considerable time. If your skin is very sensitive, or, if there are any breaks in the skin, however small, use rubber gloves. The gloves should be used also in cases where there is considerable etching to be done at one time.

[3] If several specimens are being treated, the etching solution may become considerably discolored by the products of interaction. In such cases, a fresh solution should be provided before the final etching.

The Use of the Coördinate Numbers of Meteoritic Falls in Numbering Specimens in Collections

A further use for the coördinate numbers of meteoritic falls* has been found in numbering specimens in collections of meteorites. If but one representative of a fall is contained in a collection, the coördinate number of the fall alone suffices for numbering the specimen; *e.g.*, 1205,420, for the Goose Lake, California, siderite. If, however, 2 or more representatives are included, these are readily distinguished from one another simply by adding serial numbers, assigned in any desired order by the curator of the collection, to their common coördinate number; *e.g.*, 1110,350/1, 1110,350/2, 1110,350/3, *etc.*, for members of the Canyon Diablo, Arizona, shower.

The employment of the coördinate number in numbering a specimen has several manifest advantages, *to wit:* (1) this number, being of 'cosmic significance,' is universally interpretable, whereas an arbitrarily chosen number is not; hence, if impressed upon a specimen, the coördinate number serves quite as well as would the name of the fall to identify the specimen; (2) the coördinate number constitutes, thus, an absolute means of identification, even if the label or the record of the specimen becomes lost or destroyed or if the specimen itself gets out of place and cannot otherwise be identified; (3) if a specimen bearing the imprint of its coördinate number is transferred from one collection to another, both of

*See the writer, *C.S.R.M.*, **3**, 153-5; *P.A.*, **52**, 306-7, 1944, and the various sources mentioned in ref. and n. (1) and (2) therein.

which use coördinate numbers, the only change necessary in its numbering, if any, is a change in its serial number (*ante*).

FREDERICK C. LEONARD

Large Siderite Found in the Drum Mts., Millard Co., Utah

Under the heading, "9th Largest Meteorite Found in U.S. being Analyzed Here," the following report appeared in the *Times-Herald*, Washington, D. C., of date December 22, 1944:

"The ninth largest meteorite discovered in the United States is now being analyzed in the National Museum, where it was on display yesterday. Found in Utah by two evacuees of Japanese ancestry, the burnt-sienna, iron-nickel-cobalt mass had an original weight of 1,164 lbs., E. P. Henderson, Curator of Mineralogy and Petrology, said.

"Museum experts removed a 22-lb. slice for analysis, working 56 hours to saw thru the metal. Discovery of the meteorite, which brings to 525 the number found in the United States, was made September 24 by Akio Ujihara, a graduate of Polytechnic Engineering College, Oakland, California, and Yoshio Nishimoto, both of the Topaz War Relocation Center, Utah.

"Members of a lapidary school organized at the Center, the two were hunting stones when they found the meteorite, which has a maximum circumference of 5 ft. 4 in. and measures about 2½ × 2½ × 1½ ft. Recognizing that it was no ordinary boulder, they sent a small piece to Henderson, who asked them to keep their discovery 'as quiet as possible,' so souvenir seekers would not despoil it. 'Not that there is much that could have been done to it,' he remarked. 'An iron meteorite is practically indestructible.' The site of the discovery was in Millard County, Utah, in the Drum Mountains, which have given the meteorite its name. Only 4 other meteorites have been found in the State."

The coördinate number of the siderite may provisionally be taken to be 1123,390 (this being the coördinate number of Fillmore, the county seat of Millard County).

A Microchemical Test for Nickel in Meteorites

In the January, 1945, instalment of *C.S.R.M.*, under the caption of "A Simple Test for Nickel in Meteorites," was described the well-known dimethylglyoxime test for nickel. Another excellent and extremely sensitive test for nickel, for which literally only a speck of the material that is to be tested is required, is the potassium-mercuric-thiocyanate microchemical test. This is detailed in M. N. Short's *Microscopic Determination of the Ore Minerals (Bull. 825, U. S. Geol. Survey)*, 134-5, 1931, as follows*:

"*Potassium-mercuric-thiocyanate test.*—

"Reagent: 5% $K_2Hg(CNS)_4$ in H_2O.

"Product: Nickel mercuric thiocyanate [$NiHg(CNS)_4$].

"Limit: 0.02% $Ni(NO_3)_2$ solution.

"Potassium mercuric thiocyanate added to a 1:7 HNO_3 solution containing nickel ions precipitates round spherulites that are light brown in transmitted light and white in reflected light. Sometimes an amorphous white powder is first pre-

*The dimethylglyoxime microchemical test also is given in the same work (pp. 133-4).

cipitated at the edge of the drop, but it tends to redissolve with the formation of the spherulites. The nickel mercuric thiocyanate is more soluble than the corresponding cobalt precipitate and does not form till the drop is partly evaporated. The spherulites are tinged pink if iron is present in solution. (See pl. 4, *D*.) This test is exceedingly useful in that the nickel spherulites often appear when testing for cobalt and iron."

President of the Society: LINCOLN LA PAZ, Department of Mathematics, Ohio State University, Columbus 10

Secretary of the Society: C. H. CLEMINSHAW, Griffith Observatory, P. O. Box 9866, Los Feliz Station, Los Angeles 27, California

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VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By **LEON CAMPBELL, Recorder**

The Period of W Lyrae, 181136: In the course of the discussion of the light curves of nearly 400 long-period variables there have been noted a number of instances of irregularities, not only changes in forms of light curve, but, on occasion, apparent changes in period. Attention has already been called in these notes to the changing form of the light curves of R Aurigae (see POPULAR ASTRONOMY, Vol. 50, page 279). We now have what appear to be positive changes in period of W Lyrae, at least over the past fifty years.

Since 1896 the observations of this variable have been fairly continuous, especially after 1913 when the star was placed under regular observation by the British and American variable star associations. If we plot the observations consecutively, say 1000 days in a row, they indicate approximately five maxima per 1000 days—the assumed mean period is 196.6 days—and if the rows are separated by a constant interval of magnitude, one can see at a glance the general trend of the period. If there were no change in period over the interval of time covered by the observations, any set of observations, say for 4000 days, should fit approximately over any other set of 4000 days, save for accidental errors.

Accordingly, when the curves of this star for one 4000-day interval are superimposed upon those for another similar interval of time, with the top row of one plot made to coincide with the top row of the other plot, one sees that there is a gradual shift of maxima and minima between them.

This is shown graphically in the accompanying figure (Figure 1) where all the observations were first plotted consecutively as mentioned, and then those of the last 10,000 days superimposed upon those of the first 10,000-days portion of the plot. The observations of the last 10,000 days are represented by the smoothed curve, the whole 14,000 days by unconnected dots. For the years 1913 to 1920 the B.A.A. observations were combined with those of the A.A.V.S.O. when compiling the mean ten-day curves. The top row—where the coincidence was purposely effected—though composed of somewhat meagre data, agrees well with the smoothed curve drawn through it. Immediately the maxima and minima in the successive rows begin to shift, the later observations being displaced to the left by varying degrees and then returning to normal until the ninth row is

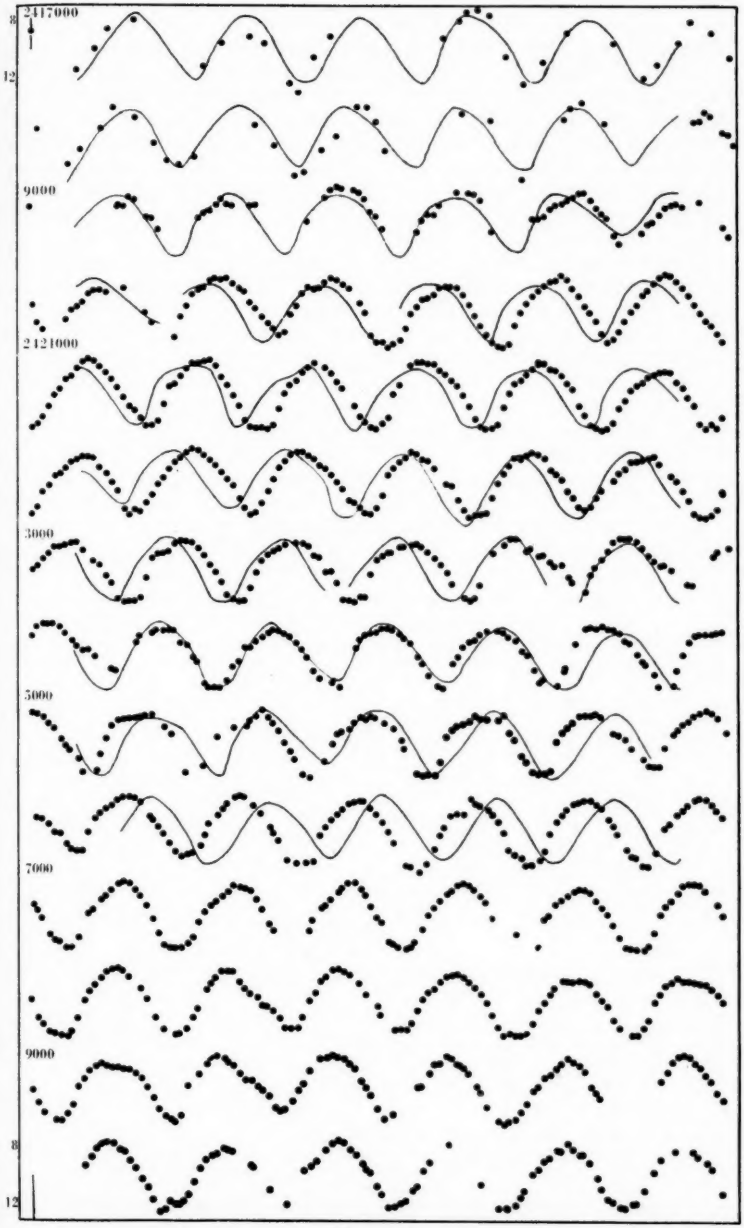

FIGURE 1
COMPARISON OF CURVES SHOWING CHANGE IN PERIOD

reached, when the shift is to the right by steadily increasing values.

A study of the mean values of the period for each group of approximately 10 maxima and minima indicates that the mean period apparently decreased from 198 days in the early 1900's to 193 days in 1925, and then gradually increased to 199 days in recent years. The mean period over the 48-year interval, derived from both maxima and minima, is 196.6 days. This is slightly in excess of the "best" value of $195^d.98$ derived by Sterne and Campbell, as published in the Harvard Tercentenary Volume, *H.A.* 105, but that discussion did not include the maxima and minima observed since 1932.

Another graphical method of indicating this apparent change in period may be seen in Figure 2 where the Julian dates of maxima and minima are indicated by dots in horizontal rows for each 1000 days. If we connect every fifth maximum and minimum, respectively, as is shown in the figure, it will be seen that the resulting curves all slope from upper right to lower left in the figure—due to the fact that the period is not exactly 200 days, an exact multiple of 1000 days, but 3.4 days less. But it is also to be noted that these slopes have appreciable curvatures, more or less similar in shape. The curve at the extreme right of the figure is derived from the mean of each row of dates of maximum and minimum, and is a smoothed curve drawn through the mean point values. The straight line shown in this curve indicates the mean slope, and it will be readily seen that the smoothed curve has a definite sinuous shape, another indication of the changing period.

In plotting the dates of maxima and minima in Figure 2, use has been made of all the published dates, each point representnig the mean around that date.

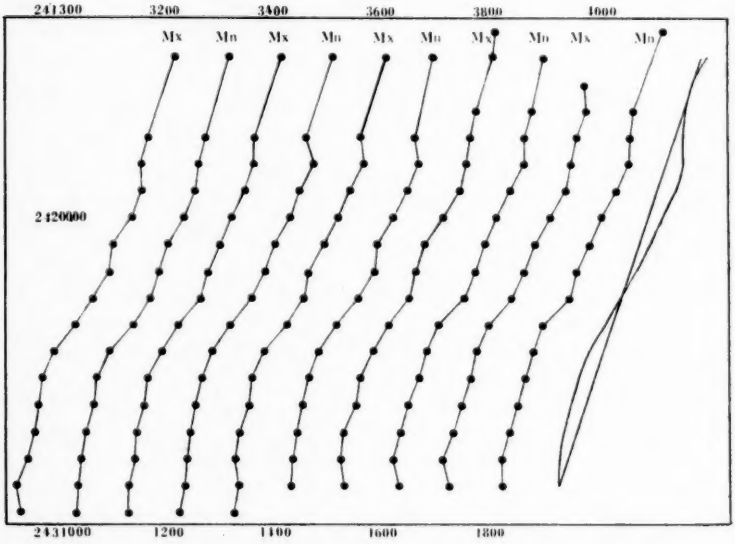

FIGURE 2

PLOT OF TIMES OF MAXIMA AND MINIMA OF W LYRAE (1896-1944)
(NOTE: The first figure in the top line should be 2413000)

The scatter about each point, where two or more observed dates are combined, is very small, the average deviation amounting to only ±3.3 days for maxima, and ±2.6 days for minima. The light curve at minimum phase is much less flat than at maximum.

The apparent change of period described in this note is probably not of the same fundamental nature as that sought for in the investigations by Sterne and Campbell. In fact in that paper the probability that the period of W Lyrae changed during the interval discussed was not found to be significant.

While individual light curves will be seen to differ slightly in form, especially at maximum, yet the over-all mean light curves compiled for 10-year intervals differ very slightly from one another. This would appear to indicate that the fundamental cause underlying the variation of the star is not materially changed over a long interval of time. These variations in form of light curves may have more reality of meaning than has hitherto been supposed to be the case.

Maxima of Bright Long-Period Variables, 1945: Predicted dates of maximum for the long-period variables which attain, on the average, a maximum magnitude brighter than 8.0, are listed herewith.

PREDICTED DATES OF MAXIMUM

Design.	Name	Date of Max. m	d	Max. Mag.
0010*32*	S Scl	12	19	6.8
001755	T Cas	8	2	7.8
001838	R And	9	1	7.0
021143a	W And	8	9	7.5
0214*03*	*o* Cet	1	15	3.8
		12	10	
022813	U Cet	4	28	7.6
		12	29	
023133	R Tri	6	7	6.3
0250*50*	R Hor	9	30	6.0
0432*63*	R Ret	8	9	7.7
0443*49*	R Pic	5	31	6.7
		11	20	
0455*14*	R Lep	8	29	6.7
050953	R Aur	10	7	7.8
0515*33*	T Col	3	5	7.5
		10	16	
054920a	U Ori	1	16	6.6
0617*02*	V Mon	5	4	7.1
0652*08*	X Mon	4	21	7.6
		9	28	
065355	R Lyn	3	25	7.8
070122a	R Gem	2	13	7.1
072708	S CMi	3	25	7.5
081112	R Cnc	4	25	6.8
081617	V Cnc	3	7	7.9
		12	3	
0824*05*	RT Hya	6	4	7.5
084803	S Hya	1	5	7.9
		9	20	
0850*08*	T Hya	2	3	7.7
		11	20	
0929*62*	R Car	2	13	4.5
		12	18	
093934	R LMi	9	20	7.2
094211	R Leo	9	2	5.9
100661	S Car	2	20	5.7
		7	23	
		12	21	
103769	R UMa	9	9	7.6
1144*41*	X Cen	5	25	7.8
1214*18*	R Crv	8	17	7.6
122001	SS Vir	3	22	6.9
123160	T UMa	3	30	7.9
		12	15	
123307	R Vir	3	7	7.1
		8	1	
		12	24	
123961	S UMa	3	29	7.9
		11	11	
1324*22*	R Hya	4	9	4.5
1327*06*	S Vir	3	2	7.1
1331*55*	RV Cen	6	23	7.6
1330*33*	T Cen	2	5	6.1
		5	7	
		8	4	
		11	5	
134440a	R CVn	10	17	7.7
1409*59*	R Cen	8	3	6.0
		12	2	
142539a	V Boo	8	3	7.9
143227	R Boo	4	19	7.3
		11	30	
151731	S CrB	8	25	7.5
1518*22*	RS Lib	1	4	7.7
		8	10	
1528*49*	R Nor	9	9	7.2
		12	10	
1536*54*	T Nor	3	8	7.4
		11	8	
154615	R Ser	8	12	6.8
154639	V CrB	5	27	7.4

Design.	Name	Date of Max. m	Date of Max. d	Max. Mag.
162112	V Oph	9	1	7.5
162119	U Her	4	17	7.6
163266	R Dra	2	14	7.6
		10	15	
164715	S Her	3	10	7.6
164844	RS Sco	4	17	6.8
165030a	RR Sco	10	1	6.0
170215	R Oph	4	14	7.6
182133	RV Sgr	4	22	7.8
183308	X Oph	1	4	6.9
		11	30	
190108	R Aql	11	1	6.2
191019	R Sgr	3	19	7.2
193449	R Cyg	10	31	7.3
194048	RT Cyg	1	25	7.4
		8	2	
194632	Cyg	3	17	5.3
194659	S Pav	6	3	7.3
194929	RR Sgr	9	27	6.7
195142	RU Sgr	4	14	7.2
		12	10	
200938	R Cyg	1	4	7.4
201139	RT Sgr	8	10	7.9
201647	U Cyg	1	14	7.6
204405	T Aqr	6	25	7.9
210868	T Cep	2	4	5.9
221948	S Gru	11	3	7.8
230110	R Peg	12	28	7.9
230759	V Car	1	3	7.9
		8	19	
233815	R Aqr	5	2	6.4
235150	R Phe	8	3	7.9
235350	R Cas	5	4	6.5

A recent A.A.V.S.O. Bi-monthly Bulletin containing a listing of predicted dates of maximum and minimum for 1945 for nearly 400 long-period variables may be secured while available by addressing the A.A.V.S.O. Recorder, Harvard Observatory, Cambridge 38, Mass.

Observations for November and December: A total of 5,935 observations were contributed by 49 observers during November and December, as listed herewith:

Observer	November—1944—December No. Var.	No. Ests.	No. Var.	No. Ests.
Albrecht	3	3	...	...
Allen	1	1	...	...
Blunck	32	37	...	...
Bouton	32	51	29	37
Buckstaff	12	27	...	...
Buechner	...	...	6	6
Chrisman	14	14	...	...
Cousins	90	192	...	...
Cragg	...	...	2	3
Dafter	5	16	...	...
Duffie	33	34	18	20
Fernald	317	725	326	649
Ford	52	61	50	58
Garneau	18	18	...	...
Halbach	29	29	...	...
Harris	36	36	21	21
Hartmann	160	190	135	139
Herbig	...	...	1	2
Holt	107	238	133	171
Houston	...	...	37	104
Howarth	14	14	17	17
Hutchings	...	...	4	15
Irland	13	17	...	...
Kearons	54	76	54	92
Kelly	11	18	11	15
de Kock	74	304	73	317
Koons	59	73	24	25
Labrecque	3	9	11	11
Lovinus	8	8	...	...
Luft	6	18	7	12
Marsh	1	1	...	...
Mary	73	157	48	102
Monnig	15	15	...	...
Nadeau	82	100	54	54
Oheim	67	173	83	139
Oravec	6	25	5	13
Parks	31	59	23	41
Peltier	98	162	88	106
Peters	26	27	...	...
Reeves	3	3	...	...
Renner	8	8	...	...
Rosebrugh	23	319	25	125
Schoenke	11	14	7	7
Sill	97	99	130	130
Stone	8	8	4	4
Stowe	...	...	7	8
Webb	15	16	...	...
Weber	34	34	50	50
Weitzenhoffer	3	4	3	9
Totals		3433		2502

January 4, 1945.

Comet Notes

By G. VAN BIESBROECK

During the year just ended four unexpected comets were discovered: VÄISÄLÄ (April?) VAN GENT (May 23), DU TOIT (May 25), BERRY (Sept. 13).

Besides these, several previously discovered and periodic comets were followed during the year. The most unfavorable return of periodic comet ENCKE went by last summer without the comet being observed. There have been no reports of better luck toward the latter part of the year when southern observers might have tried to find it as an evening object.

As the new year opens there are no known comets under observation except PERIODIC COMET SCHWASSMANN-WACHMANN 1 which shows large fluctuations in brightness but is generally faint. It has been observed here only occasionally.

During the year three periodic comets are due to return to perihelion:

COMET JACKSON-NEUJMIN, Perihelion	1945 April 25
PONS-WINNECKE	July 8
KOPFF	August 9.

As the time of visibility of these comets approaches, ephemerides will be given in these notes. The first of these objects will be unfavorably located but the two others should become well visible.

We should add that there is a good chance that the faint periodic comet 1943*a* (OTERMA) will be recorded around the time of its opposition, July 16.

Williams Bay, Wis., Jan. 15, 1945.

General Notes

F. Wagner Schlesinger, director of the Fels Planetarium in Philadelphia, has been appointed director of the Adler Planetarium in Chicago. (*Science,* January 12, 1945.)

Dr. Roy K. Marshall, according to announcement by Dr. Henry Butler Allen, Secretary and Director of The Franklin Institute, has been appointed Associate Director of the Institute, in charge of astronomy, photography, and seismology, to succeed Wagner Schlesinger when he takes over his new duties as director of the Adler Planetarium in Chicago, February 1. Dr. Marshall also succeeds Schlesinger as Director of the Fels Planetarium of The Franklin Institute.

Dr. Marshall, who has been assistant director of the Fels Planetarium since September 1, 1939, is a graduate of Ohio Wesleyan University and the University of Michigan, having received his Ph.D. degree from the latter institution in 1932. He is the author of numerous papers and books on astronomy, including the monthly Fels Planetarium booklets. Married and the father of three sons, he lives in Narberth, Pa.

Dr. Eckert Heads Pure Science Department

Dr. Wallace J. Eckert, Head Astronomer and Director of the Nautical Almanac Office at the United States Naval Observatory at Washington, D. C., and formerly professor of astronomy at Columbia University, has been appointed director of the Department of Pure Science newly organized by International

Business Machines Corporation, it was announced by the company recently. In this post Dr. Eckert will be stationed at the IBM World Headquarters Building in New York.

The new position has been created by Thomas J. Watson, IBM president, to further the company's program to advance scientific calculation. For many years Mr. Watson and the company have made extensive contributions to scientific calculation, and it is their desire to follow out this tradition still more effectively in the future, it was stated.

Dr. Eckert is to assume his new post with the company on the completion within a few weeks of the 1946 volumes of the *American Nautical Almanac* and the *American Air Almanac*, and of the *American Ephemeris* for 1947. During the five years in which he has been Director of the Nautical Almanac Office, Dr. Eckert has been responsible for these publications and has contributed to the development and examination of new methods of navigation, as well as being the Navy's chief consultant on astronomical matters.

Before going to the Nautical Almanac Office, he was professor of astronomy at Columbia University, in which post he organized the pioneer scientific computing laboratory which later was administered by the Thos. J. Watson Astronomical Computing Bureau. Dr. Eckert has been a member of the Bureau's board of managers since its organization and is author of "Punched Card Methods in Scientific Computation." His principal field is celestial mechanics and astronomy of position.

While at the Naval Observatory, Dr. Eckert has been responsible for many new developments in navigation, including the present *Air Almanac*, the new sky diagrams and methods of emergency navigation for ships and aircraft. The *Air Almanac*, of which millions of copies have been printed, is accepted as a major contribution to air and surface navigation alike. Sky diagrams make it possible for a poorly trained navigator to select at a glance the most suitable objects for his purpose and to identify them in the sky.

Lifeboats of the Merchant Marine are equipped with a pocket-size publication which serves the combined purpose of all of the navigational volumes usually carried on a plane or ship. A pocket bubble sextant developed by Dr. Eckert for emergency use enables one without special training to determine his position within a few miles.

The *American Air Almanac*, which appears in three volumes for each year, is used for all celestial navigation in Army and Navy aircraft and on ships. The *American Nautical Almanac* is used on all the ships of the Navy and Merchant Marine. The *American Ephemeris* and *Nautical Almanac* is the basic volume from which the nautical almanacs are prepared. It contains astronomical predictions of the highest accuracy and is the standard source of such data for astronomers, engineers and other scientists.

During the five years in which Dr. Eckert has been at the Naval Observatory revolutionary methods of computing, printing and proofreading have been introduced which give the office foremost place in its field. With these new methods it has been possible under wartime personnel difficulties and failure of foreign cooperation to take on additional tasks, and at the same time maintain the standards of the regular publications and to advance their publication dates. (From information sent by E. C. van Dyke & Co., 55 Liberty St., New York 5, N. Y.)

New York, January 20, 1945.

The Cleveland Astronomical Society

Dr. G. Van Biesbroeck of the Yerkes Observatory spoke to the society at the January meeting on "Stellar Systems." The theme of his lecture was that stars come in systems. He pointed out that many of the stars, when carefully examined with a suitable telescope, prove to be binary stars forming a system the mass of which may be obtained if the period of revolution and distance are determined.

It was, indeed, a great pleasure to hear the man who is probably the only person in the northern hemisphere at the present time actively engaged in double star work tell about them.

The part the spectrograph has played in detecting binaries too close to be observed visually was pointed out and also how the work on binaries has helped to explain types of variable stars, the eclipsing binaries such as Algol.

From binary stars the lecturer led on to triple and multiple stars, then to star clusters of open and globular types, then to our Milky Way or Galactic System and finally to other Island Universes always reminding us that stars come in systems.

The following afternoon, January 6, the Ohio Neighborhood Astronomical Group had the pleasure of hearing Dr. Van Biesbroeck speak on "Faint Luminosity Stars." He told the group about his work in trying to determine the lowest limit of magnitude. The faintest star he has thus far found has an absolute magnitude of 19.2.

Interesting discussions were carried on after the lecture in which questions from various fields were answered including some on comets, another field in which Dr. Van Biesbroeck is prominent. The meeting was followed by a dinner for the distinguished guest.

HENRY F. DONNER.

3094 E. Overlook Rd., Cleveland Heights 18, Ohio.

Summary of Sun-Spot Observations at Mount Holyoke College, 1944

Month	No. of Obs.	North of Equator No. of Groups	North of Equator Av. Lat.	South of Equator No. of Groups	South of Equator Av. Lat.	Av. No. Groups at one Obs.	New Groups
January	11	1	+ 9°.0	0	....	0.7	1
February	21	1	9.0	0	....	0.0	0
March	20	1	7.0	3	—13°.4	0.8	4
April	16	0	....	0	....	0.0	0
May	21	1	2.0	1	—22.0	0.2	2
June	23	2	2.4	1	25.0	0.4	2
July	27	2	17.5	3	28.2	0.4	5
August	20	3	29.2	5	13.4	1.6	8
September	18	1	20.6	3	19.2	1.0	4
October	26	2	19.8	7	19.1	1.7	8
November	16	2	20.0	5	17.1	1.2	6
December	23	6	22.4	4	19.0	2.2	10
Totals	242	22		32			50

Average number of groups at one observation.....0.88
Average latitude of groups north of the equator.....+18°.8
Average latitude of groups south of the equator.....—19°.0
Number of days without spots.....109

The number of observations during the year is the highest yet and indicates that weather conditions interfered on 27% of the days when an observer was on hand.

The sharp increase over the previous year in average latitude (10° in both hemispheres of the Sun) mirrors the fact that in 1943 only 5% were new-cycle groups, in 1944 probably 70%. As previously noted, three high-latitude (—39°, —25°, —24°) groups appeared in the southern hemisphere in 1943 (May, October, and December), and others have followed in rapid succession in 1944. By contrast, no new-cycle group was recorded north of the solar equator until July, 1944 (at +29°).

On the basis of these annual figures, this minimum is a high one. The Sun was "clean" on only 45% (65% on the two previous occasions) of the days of observation; the number of new groups exceeds those seen in 1933 and 1923 by an average factor of 2.7; and the average number at one observation is more than twice as great.

ALICE H. FARNSWORTH.

John Payson Williston Observatory, South Hadley, Mass., January, 1945.

Fifty Years Ago

In looking through the issue of POPULAR ASTRONOMY for February, 1895, (fifty years ago) our attention was arrested by an article bearing the title

MARS.

PERCIVAL LOWELL.

THE CANALS. I.

It proved to be one of the earliest authoritative statements relative to a topic which was destined to be under discussion for many years to come. Since very few present-day readers of this magazine were readers of it fifty years ago, we are reprinting portions of the paper here for both its intrinsic interest and its historical value.

> In the autumn of 1877 the well-known Italian astronomer, Schiaparelli, while engaged in micrometric measurements of the markings on the planet, made a strange discovery connected with them; so strange that his announcement of it was at once received with disbelief. He found that the bright areas of the planet's surface were traversed by narrow dark lines linking the bluish-green portions of the disc to one another in a most singular manner. As the bluish-green regions were at that time regarded as bodies of water, he called the lines canali or channels, which was translated canals and became the now famous canals of Mars.
>
> Such a system of aquatic communication—a Venice on a world-wide scale—was something without counterpart either in the Earth beneath or in the heavens above; and as it failed to fit either terrestrial or celestial experiences, it was with one consent refused recognition.
>
> The general scepticism was furthered by the fact that no one but their discoverer could see these strange discoveries of his. Not only did no one else perceive them independently in 1877, but even after others knew what to look for and tried to make out the phenomena not a man of them succeeded in doing so. Markings which can now be shown to have been badly seen views of the canals were, it is interesting to note, observed by Schroeter at the very beginning of this century and by several observers since. But what is very good evidence after the fact is no evidence at all before it. These previous adumbrations of the canals were not then regarded as having anything to do with the case and carried therefore

no conviction. To the same inconclusive category belong the long streaks seen and drawn by Burton and Dreyer in 1879.

So scepticism continued, while Mars came and went. Opposition succeeded opposition, each adding to Schiaparelli's discoveries on the one hand and to distrust of them on the other. For nine years this state of incredulity lasted; and not a scrap of confirmation did his discoveries call forth except from himself—which rather invalidated them than otherwise. Discredit was simply heaped on discredit; such being the world's way of appreciating a man who is ahead of his times. To be previous is to be pooh-poohed.

Meanwhile, the longer scepticism lasted the more was it laying up for itself against the day of its recantation. For Schiaparelli went from surprise to surprise. His first drawings in 1877 were not, as it seems to us now, so very startling. In 1879, however, his delineations became much more so, and in 1881 he announced that some of the canals had geminated; that is, that in place of a single canal there were now two parallel ones, running side by side often for more than a thousand miles. Such a Martian parody on railroad tracks, as it were, capped the climax to the general distrust. No one else, it will be remembered, had yet succeeded in seeing single in the matter.

How long the rest of the world would have remained in nescience of the canals had it been left to its own devices, will never be known; for the earliest confirmation Schiaparelli received was at the hands of such of his scientific friends as had been spurred by him to the attempt. The first to succeed was Perrotin at Nice in April, 1886, after as nearly missing it as it was possible to come.

The occasion of his attempt was the completion of the great Nice refractor of 29 inches aperture. The great Washington refractor of 26 inches had already failed to show them, whereas Schiaparelli had discovered them with 8⅓ inches. An instructive comment this on the popular superstition about the superiority of large instruments; a superiority in reality quite secondary in matters of planetary detail to both observer and atmosphere.

Perrotin had, he tells us, made a first series of attempts to see the canals which was not encouraging and after a few days of fruitless endeavor, explained partly by the bad seeing, partly by the difficulty inherent in this class of investigation, had given the search up, then resumed it and was about to abandon it for good, when on the 15th of April he managed to make out one of the canals between the Syrtis Major and the Sabæus Sinus, the Phison. Mr. Thollon saw it immediately afterward. We can conceive the thrill that must have gone through those observers at the sight. For here was something which only one man had ever seen before, and which the world had denied to exist for nine years. The verifiers knew moreover the interest that would follow their announcement of the sight. For it is not the discoverer usually whom the world believes but the man who first sees the sight second.

In this case, however, in spite of their confirmation many astronomers still refused to be convinced and much discussion was indulged in as to the cause of the self-deception on the part of the alleged observers. All of which reads inapropos enough now. For time has brought ample testi-

mony to the keenness and accuracy of Schiaparelli's discovery. Since 1886 each opposition has added new names to the list of those who have seen the canals with their own eyes. So that though sceptics still exist they form now the antiquated minority.

The reader will doubtless wonder how such a state of things was possible, that the many should all turn out wrong and the one right. Primarily, of course, this was due to Schiaparelli's extraordinary keenness of vision, but it was supplemented by a certain as uncommon keenness of brain. Schiaparelli possessed the very rare faculty of perceiving what he saw. With him the eye worked the sensorium of its own accord. Commonly the brain has to be warned beforehand. Most people see only what they are prepared to see; as is well instanced in astronomy by those observers who manage to mark with surprisingly small instruments what others have already discovered, and yet who make no discoveries of their own.

But there is a second fact ancillary to the first. The essentials to good observation are not understood to the perfection that they ought to be. I may mention here one or two very simple points out of many.

Here follow several paragraphs in which the author points out that success in seeing the canals depends upon the use of a refractor rather than a reflector, an unusually steady atmosphere, and the "capacity for taking infinite pains" on the part of the observer. In the absence of drawings he devotes a few paragraphs to a verbal description of the appearance of the canals which were recognized at that time. He then continues. Ed.

Now one characteristic of the canals is as important as it is striking.

At certain times the canals are invisible; and their visibility or invisibility has nothing whatever to do with our atmosphere or with that of Mars. It is a characteristic inherent in the canals themselves. Furthermore it follows dutifully upon the Martian seasons. In the late winter or early spring of the southern hemisphere the canals are invisible with the telescopic power we now possess. As the spring advances they begin to show as very fine lines that with bad seeing masquerade as broad, faint streaks. From this time on they gradually darken and broaden somewhat till they have attained their maximum width of about fifty miles or less after which the process is reversed and they disappear again. In short the canals grow.

During their evolution they are rather more conspicuous at their debouching ends, rather as if they were a trifle larger at those points than as if development began there. But this difference is almost imperceptible. The striking thing is that practically they develop simultaneously throughout their whole length if this does not exceed a thousand miles.

They do not all begin to develop at the same season. Those nearest the south pole start first. The Solus Lacus region is the one to lead off the list. Then the others follow in their order north. Proximity to the dark regions, however, seems to further the process and those near such areas begin before the time their latitude strictly would warrant. Such is the case with the canals about the Sabæus Sinus and still more markedly with the Ganges.

Development does not immediately follow the transference of the polar water north. Some weeks elapse after the water has to all appearance gone down the disk before the canals appear; a delay of just about the length of time it would take vegetation to sprout. What this hints as to the probable character of the canals we shall see in the next paper when we come to take up some still more recently discovered phenomena intimately connected with the canals—the oases.

LOWELL OBSERVATORY, DECEMBER, 1894.

Book Review

The Metallography of Meteoric Iron, by Stuart H. Perry (United States National Museum, Bulletin 184, 1944, 206 pp. and 81 plates, $0.60).

The chief purpose of Bulletin 184 is well described by its author in the words: "The present work aims to apply metallography, both theoretically and practically, to the study of iron meteorites." Perry's interesting and scholarly volume will also serve most effectively to confute those who are accustomed to describe as *classical* all investigations into the microscopic structure of meteorites. Readers of his monograph will speedily discover that *modern* meteoritics is not solely a physico-mathematical science. On the analytical-descriptive side, in the application of the most recent and refined microscopic and chemical techniques, meteoritics has been kept fully abreast of the times by the studies of Perry and the other investigators, American and foreign, whose contributions make up the subject matter of his book.

Bulletin 184 begins with a concise but meaty summary concerning the classification of iron meteorites and the nature and characteristics of their primary and accessory constituents. Siderites are subdivided into only three groups—the hexahedrites, the octahedrites, and the ataxites—although the existence of forms transitional between octahedrites and both hexahedrites and ataxites is recognized. Not only the primary constitutents of siderites—kamacite, taenite, and plessite—but also the accessory constituents—troilite, daubréelite, schreibersite, cohenite, and the oxides, silicates, and minor constituents—are described in detail with particular emphasis on structures requiring for their correct interpretation a knowledge of modern metallography. Succeeding chapters on *Artificial Irons, Alloys, The Iron-Carbon System,* and *The Iron-Nickel System* lay the foundation for such interpretation by familiarizing the reader with the *methods* (*e.g.*, micropolishing, microetching, electrolytic etching, heat tinting, sulphur printing, the Perry quantitative nickel coloration process, spectroscopic and X-ray analyses, and identification processes based on the use of polarized light) and the *terminology* (*e.g.*, phases, thermal critical points, hysteresis, solid solutions, eutectics, eutectoids, paraeutectoids, phase equilibrium, equilibrium diagrams) of technical metallography. Detailed interpretations of meteoritic structures are then given in eight chapters on *Meteoric Nickel-Iron, Hexahedral Structures, Octahedral Structures, Plessite and Nickel-Rich Ataxites, Heat Alterations, The Role of Phosphorus in Meteoric Iron, Carbon and Sulphur in Meteoric Iron,* and *Neumann Lines and Deformations.* From the viewpoint of the meteoriticist, these eight chapters constitute the chief but by no means the only merit of Perry's book. The reader, struck by the beautiful simplicity of the iron-carbon diagram

of an earlier chapter, may expect an easy conquest of meteoritic structure problems by means of the metallographic approach. Perry, anticipating this unduly optimistic frame of mind on the part of his readers, carefully points out that for several reasons (*e.g.*, the marked hysteresis of iron-nickel alloys and the wide range of nickel content within which structural changes of iron-nickel alloy can occur) even the *artificial pure* iron-nickel alloys pose more difficult problems than those encountered by the metallographer in his investigation of the iron-carbon alloys of industry. As regards the *natural* iron-nickel alloys, contaminated with phosphorus and sulphur, which are the concern of the meteoriticist, the situation is far more complex and an additional major difficulty is encountered which is well described in the following quotation from Perry:

"In the artificial iron-nickel diagram the transformation points are determined positively from the behavior of alloys of definite composition under controlled conditions; but in meteoric iron only the final cooled product can be studied, and the transformation points can merely be inferred from the observations of various investigators upon irons of varying compositions."

In spite of the complexity and indeterminateness emphasized in the immediately preceding remarks, the modern metallographic attack on meteoritic structure problems has led to most important advances. Among the more striking conclusions reached by Perry and other workers in this field are the following: hatched kamacite, formerly attributed to the development of Neumann lines "is a product of phase transformation"; the characteristic oriented sheen of kamacite, for which a number of untenable explanations have been given, is now regarded as arising chiefly "from the influence of atomic planes revealed by the etching process"; although taenite is always described in meteoritical literature as strongly magnetic, a given specimen will be non-magnetic at room temperature if the conditions of the phase transformations which produced the specimen are such as to lead to a nickel content falling between approximately 26% and 30%; plessite, hitherto regarded as exclusively an octahedral structure, has been found by Perry in both Otumpa and Sierra Gorda; the isometric nature of cohenite crystals is open to question; the earlier theory of an iron-nickel eutectoid is now generally regarded as untenable; on the basis of the iron-nickel diagram, it is possible to give a satisfactory outline of the essential mechanism by which the members of the "trias"—kamacite, taenite, and plessite—and therewith the various types of siderites are produced; the authenticity of Octibbeha (nickel content = 62%), long regarded as doubtfully meteoritic, appears to have been established by Perry; characteristic Widmanstätten structures on a microscopic scale have been produced artificially by Benedicks and by Mehl and Derge; the nature of kamacite needles, long the subject of controversy, has been established; and the mechanism by which the so-called "swathing kamacite" develops has been satisfactorily explained, at least in the case of the kamacite bands surrounding nodular masses of troilite.

The book under review closes with a stimulating chapter on *Metallographic Techniques*. The detailed instructions given in this chapter and the accompanying large collection of superlative photomicrographs, which furnish compelling evidence of the effectiveness of the procedures described by Perry, cannot fail to materially affect for the better all future studies of the siderites.

Readers will close *The Metallography of Meteoric Iron* with the conviction that the powerful techniques exemplified in it must be adopted universally as the standard procedures for the study and interpretation of the host of siderites

that will be discovered when systematic searches with properly designed meteorite detectors are instituted. Furthermore, those readers who are most familiar with the microscopic, densitometric, and chemical methods of the founders of meteoritics will approve most heartily of Perry's recommendation that a *restudy* be made of all irons not recently investigated by modern metallographic methods.

In concluding this review, attention is called to the following minor slips or oversights noted in reading Bulletin 184: p. 1, footnote 1. In the reviewer's opinion, it is to be regretted that Perry has chosen to speak of *meteoric* rather than *meteoritic* iron. To the meteoriticist a *meteorite* is an enduring material object, a *meteor* is a transient, luminous phenomenon engendered by the passage of a meteorite through the atmosphere. During the very brief period of meteoric incandescence, only the most superficial changes occur in the iron-nickel alloy of a siderite. If the basically sound usage of the meteoriticist is followed, use of ". . . the term meteoric iron because its composition and structure was established while the mass was a meteor" is precluded; p. 29, up 15. Use of the term *eutropic* (introduced by Rinné in 1905) in place of *eutectoid* would seem preferable since implication that the degree of fusibility is pertinent is undesirable; p. 32, up 1. In place of *cementite* read *pearlite;* p. 43, up 12. In place of *iron-carbon* read *iron-nickel;* p. 60, down 11. In place of *meteors* read *comets;* p. 75, up 20. In place of *plate 67* read *plate 74;* p. 86, up 12. Interchange the words *sulphide* and *alpha-iron* to conform to p. 86, up 6-8; p. 105, Chap. XVIII. The useful bibliography compiled by Perry should contain reference to the published *Appendices* to Prior's *Catalog,* viz., (First) *Appendix to the Catalog of Meteorites* by G. T. Prior (1927) and *Second Appendix to the Catalog of Meteorites* by Max H. Hey (1940); p. 150. Figs. 1 and 2, plate 30 are rotated through 180° with respect to the orientation shown in the related fig. 4, plate 29; p. 204. The definition of *rhabdites* is given on p. 18, not p. 19.

Lincoln La Paz.

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